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Volume 4, Issue 23 • 1999

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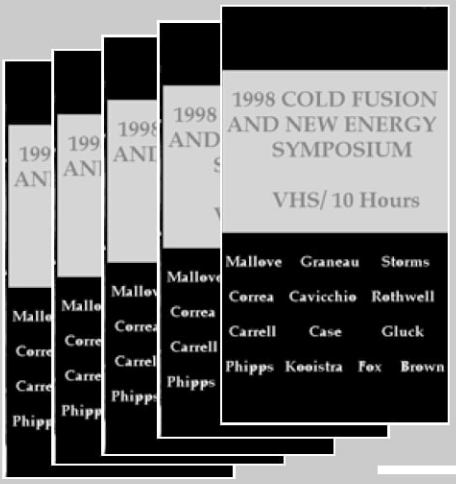

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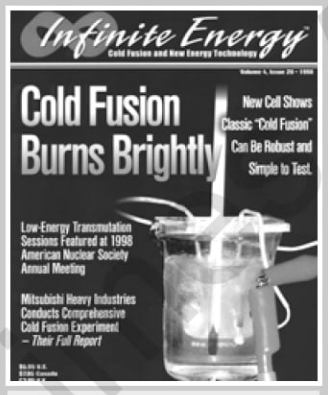

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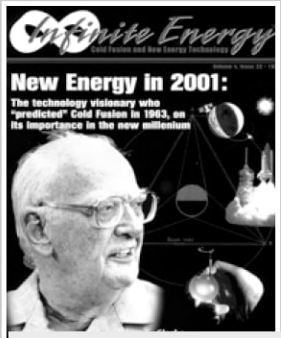

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August 1993 *Popular Science* $10. Inscribed by Eugene Mallove

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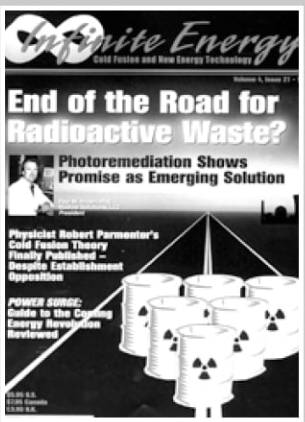

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Cold Fusion and New Energy Technology

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About the cover: A fanciful schematic description of deuterium nuclei that are undergoing nuclear fusion. Two heavy hydrogen nuclei produce a helium-4 nucleus on or near the surface of a palladium-doped, activated carbon surface. This is the "catalytic fusion" process of chemical engineer Dr. Les Case, whose work appears to have been replicated at several independent labs. *(Cover Art: Jeremy Slayton; SEM Photo: Courtesy T. Mizuno)*

Volume 4, No. 23
January/ February 1999

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INFINITE ENERGY Magazine (ISSN 1081-6372) is published six times a year by Cold Fusion Technology, Inc., P.O. Box 2816, Concord, New Hampshire 03302-2816. Subscription price: $29.95 U.S. and Canada, $49.95 other foreign. POSTMASTER: Send address changes to INFINITE ENERGY, P.O. Box 2816, Concord, NH 03302-2816.

INFINITE ENERGY Magazine
P.O. Box 2816
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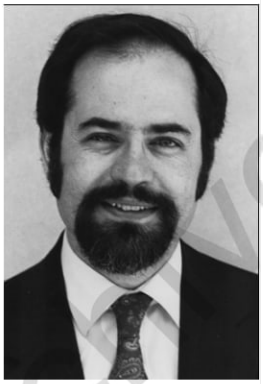
By Eugene F. Mallove, Sc.D.

Cold Fusion: Fire from Water



History will record that in the closing decade of the second terrestrial millennium in the common era, human beings began to "burn water" for their primary source of energy.

Thus was the 130 year-old prophecy of Jules Verne fulfilled: "I believe that water will one day be employed as a fuel, that hydrogen and oxygen which constitute it, used singly or together, will furnish an inexhaustible source of heat and light, of an intensity of which coal is not capable. . . I believe then that when the deposits of coal are exhausted, we shall heat and warm ourselves with water. Water will be the coal of the future."—*The Mysterious Island*, Jules Verne, 1870.

The water "burning" that will transform global civilization will not be the crude chemical burning of hydrogen that so enthralls some environmentalists and fuel cell advocates the world over. That kind of hydrogen burning requires another source of energy to split the water molecule—whether that source be solar power, fossil fuel, or nuclear fission. Nor will it be obscene, last-gasp diversionary technologies such as the splitting of hydrogen from hydrocarbon molecules for use in fuel cells aboard Toyota or Chrysler cars. Just what we need—hydrogen-powered cars running on gasoline!

The technologies that will utterly vanquish the planet's hydrocarbon combustion empires will be the lineal descendants of that remarkable "cold fusion" announcement on March 23, 1989 at the University of Utah. These new energies will come in several "flavors."

One of the most promising "new hydrogen energy" technologies is "catalytic fusion," which is being pioneered in laboratories from New Hampshire to California. It is really a very elegant embodiment of the original Fleischmann-Pons idea, which packed heavy hydrogen nuclei into palladium electrolytically. That electrochemical method suffered from problems in reproducibility owing to materials issues in bulk palladium cathodes.

The pioneering work of Dr. Les Case of Fusion Power, Inc. of Greenland, New Hampshire, which we have profiled in past *Infinite Energy* issues from No. 19 onward, now appears to have been independently validated in a number of laboratories. It began with work at SRI International performed by Russ George and now continued by Dr. Michael McKubre with catalytic funding, we understand, from a defense research agency (DARPA).

The process is elegant. Heavy hydrogen gas—deuterium gas—in contact with carbon catalysts doped with less than 0.5% by mass palladium, seems to do the trick just about every time the process is tried. As long as the correct catalyst is used and the heat transfer geometry is right, an apparent heat-producing nuclear fusion reaction is catalyzed and helium-4 nuclear ash emerges. Absent are the deadly gamma rays that would be produced in multi-million degree plasma fusion, were this very rare reaction path to helium-4 to occur. What has not yet happened is that blessed self-sustaining mode, in which the reaction, once triggered by auxiliary input, continues by itself. Les Case continues to work toward this critical goal. To the paradigm blinkered conventional physicists, catalytic fusion may be an unthinkable alchemical heresy, but it is happening right now.

Read the interviews with Drs. Case and McKubre in this issue to learn more about progress in catalytic fusion. These were obtained in the course of filming for our forthcoming cold fusion video documentary: "Cold Fusion: Fire from Water." (We expect that this one-hour video program will become available within the first quarter of 1999.)

Another promising water "burning" technology that was triggered by the Fleischmann-Pons announcement is the high-temperature, gas-phase process being pioneered by Dr. Randell Mills at BlackLight Power, Inc. of Malvern, Pennsylvania. In an extensive interview in *IE* No. 17, Dr. Mills explained his non-nuclear, catalyzed hydrogen-shrinkage reaction. This might be thought of as "superchemistry"—so super that a 200 HP car with a 20-gallon tank of water might travel 100,000 miles. This is a bit more tame than the 55 million miles that a "catalytic fusion"-powered car would travel, but there is plenty of water to go around. Neither process has hazardous by-products—shrunken hydrogen atoms or "hydrinos" from the Mills process, toy balloon variety helium for "catalytic fusion."

In this issue's "Briefs," you will learn that Mills and his staff, flush with new venture capital support, will be relocating to the Princeton, New Jersey area. We predict a catalytic meltdown of the infamous Princeton Plasma Physics Laboratory hot fusion reactor when Mills and his boys roll into town. Maybe sooner. BlackLight Power, Inc. has let the world know on its www site that in January 1999 (after we have gone to press with this issue), it will announce a "revolutionary breakthrough." We hope that this will be the long-awaited heat-producing demonstration device. If the world is lucky, it might even be scaled already for electric power generation.

We won't be greedy. Any form of tangible, irrefutable physics-busting technology out of BlackLight Power that "burns" water will be the death-knell for the business-as-usual energy sector. Those who have ignored the signs of impending energy revolution over this past decade are advised to off-load their hydrocarbon-combustion stocks—pronto!

Exxon-Mobil *et al.* will soon look back fondly on the days when crude oil was at its recent highs near $10/barrel. The emergence of water-burning technology will eventually (perhaps quickly) lead to the price of crude oil dropping to only a few dollars a barrel. This happy event will transpire as the oil cartels—what's left of them—try to dump their remaining reserves on a world market that increasingly will not be needing them (not for combustion, at least). Isn't it wonderful that even in late 1998—before widespread understanding of the new hydrogen energy revolution—gasoline prices in the U.S. have plunged so dramatically? In some U.S. locales the non-tax portion of a gallon of gasoline is approximately the retail cost of bottled spring water! A profile of the future, to be sure.

This issue of *Infinite Energy* has many other articles that are pregnant with promising science and technology. We have the work of Chip Ransford at Nova Resources Group, who took the initiative to explore the Mizuno-Ohmori plasma-electrolysis process in depth. Using ultra-pure carbon rods in a very clean cell, he has obtained both apparent over-unity

Editorial continued on page 5

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Editorial continued from page 3

energy production and apparent readily obtainable transmutation products. These are ferromagnetic materials coming right out of the water and potassium carbonate electrolyte. See Ransford's "Non-Stellar Nucleosynthesis (p. 16)."

From Italy, Dr. Elio Conte provides a clever experiment involving beta-particle bombardment of a tiny aluminum cathode. He describes this experiment in detail sufficient for many others to check it out. Low-level neutron emission is seen that is correlated perfectly with the meltdown of the cathodes under water. Perhaps this sheds light on the fundamentals of some of the cold fusion reactions. Dr. Conte believes the reaction is an electron-proton fusion. He offers his theoretical insights on the process. We will be hearing more from him in later issues.

Dr. Ruggero Santilli continues his series on the theoretical development of his Hadronic Mechanics as applied to new energies—a prelude to confirming experimental results that he will provide in a future issue of *Infinite Energy*. He is a pioneer in thinking about the shortcomings of quantum mechanics. His work should be examined carefully by those with the talent to do so.

We also report on the astonishing Japanese experimental results involving anomalous hydrogen production from the stirring of water with cuprous oxide catalysts. This work might provide some insight into past claims of excess energy production in water cavitation devices—including our own positive results in 1993 with the HydroSonic Pump. Unfortunately, we must at the same time now report negative findings with the Kinetic Furnace, after expending considerable time and energy attempting to coax the machine into life. We do not shirk our responsibility to report failures as well as successes, but failures in one technology—perhaps temporary even in that one—do not detract from what we are convinced is inevitable: a breakout in new energy technology in the early years of the new millennium.

Who has been in the vanguard of keeping information about the impending "Fire from Water" age from the citizenry? Why, none other than those self-righteous denizens of the academia-Government complex, the paragons of pontificating puffery at "CSICOP." Read about them and weep in our expanded "Words to Eat" column in this issue. For a decade now, these bigoted "science cops" have maligned honest, frontier research in new hydrogen energy and low-energy transmutation. They have been asking for mighty slaps across their smiling faces, and we have delivered the first of many to come—unless an unexpected outbreak of intellectual integrity

Editorial continued on page 8

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LETTERS TO THE EDITOR

Dear Editor,

Thanks for the highly readable articles about that giant of our time, Arthur C. Clarke! I'm glad, though, that in your own you included the caveat that "Clarke can sometimes fall short," and must point out an apparent shortfall revealed (and shared, alas) by Jed Rothwell in the article following.

Both apparently deplore the automobile, on the grounds that it wastes resources. Clarke is approvingly quoted as writing "no sane society would tolerate" them!

That smacks, I fear, of eco-fascism.

Perhaps Clarke and Rothwell are not well acquainted with free-market economics. If they were, they would acknowledge that the price mechanism perfectly well disposes of any allegation of "waste." If millions of "insane" members of society decide in their individual, sovereign wisdom to spend their own money to enjoy the freedom of the road—something never previously possible for more than an elite in the human race—not even Arthur C Clarke is entitled to blame them.

That does not say, mind you, that I call for roads to be "free" in the sense that taxpayers should be obliged to fund them. Political intervention, either favoring or inhibiting car users, is what should not be "tolerated."

Jim Davies,
Newbury, NH

❄ ❄ ❄

To the Editor:

Solomon Goldfein's article, "Energy Development from Elemental Transmutations in Biological Systems," in Issue No. 18 of *IE* brings up a curiosity about human metabolism. It is common knowledge that the average adult's caloric intake is 2000 Calories per day and it is somewhat lesser common knowledge that a clothed adult in a 20 °C environment radiates about 300 Watts. The Calorie used for food and nutrition is spelled with a capital "C" to denote a different definition of the calories: 1000 scientific calories to one Calorie. At 4.184 Joules per calorie, 2000 Calories equals 8,368,000 Joules. Since one Joule per second is a Watt, and there are 86,400 seconds per day, this is about 97 Watts. Is the discrepancy between 300 and 97 Watts just poor approximations or does the human body typically radiate more energy as heat than it takes in as chemical energy in its food? Do human mitochondria transmute elements and generate heat in addition to the oxidation of our food's carbohydrates, fats, and proteins? Who wants to do a calorimetric study on this?

Ken Rauen
West Warwick, RI

You are most likely in error on the 300 watts for an adult human. It is more like 100watts. Still, we wonder about those mitochondria!—EFM

❄ ❄ ❄

To the Editor:

As the millennium begins, we will find ourselves closer to understanding new energy systems. I would like to propose a combined conference in the year 2000 that would include the International Conference on Cold Fusion (ICCF), the Aether Physics Conference, the Saint Petersburg Fundamentals of Natural Sciences Conference, Infinite Energy Cold Fusion and New Energy Symposium and the INE International Symposium on New Energy (ISNE).

Toby Grotz
Craig, CO

❄ ❄ ❄

Sirs:

I would like to assure your readers that "Jeremy," as the reviewer of his book "A Theory of Everything," Eugene F. Mallove, Sc.D,—familiarly refers to him, is not suffering "severe pyschological trauma" because of his belief that cold fusion in the form discussed in your magazine is barking nonsense. As for the mill and its grist, if one of the apparati discussed in your publication produces enough energy by cold fusion to run my toaster I will not only eat my words but also the toast.

Yours,
Jeremy Bernstein

Dr. Bernstein,

Delighted with your letter (about our "Words to Eat" column, *IE* No. 22). We shall print it. It is exactly the kind of response from you that I had predicted.

"Barking nonsense"—now that is a good one for history! Almost as good as "scientific shlock"—by hot fusion Prof. Ronald Parker of MIT, who uttered these words about cold fusion on April 28, 1989 even before he had analyzed the data from his CF experiment. Sorry that you take your cues from folks like that and the propagandists—*e.g.* Taubes—who they influenced.

We'll have some toast ready for you in due course—"after" the toaster goes down. . .

Correction: The psychological trauma comes "later." Now you're riding high on your ego.

Sincerely,
Dr. Eugene F. Mallove,
Editor-in-Chief

❄ ❄ ❄

Dear Dr. Mallove:

I thank you very much for publishing and sending the beautiful book (*Nuclear Transmutation: The Reality of Cold Fusion*). It is really good for other researchers of the cold fusion study who have continued the very difficult work. And my father, who is now 80 years old,

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was waiting to see the book. I am going to show it to him.

I think the situation surrounding the cold fusion study is now changing, because the reaction, including excess heat evolution, can be replicated rather easily. Moreover, according to the reaction products, the transmutation materials are completely generated by glow discharge method in the electrolyte. However, we have to solve the mechanism of the reaction. It can be considered that the most reasonable theory that shows the complicated reaction may be electron capture into nuclei. I really hope that this study will give many possibilities and encouragement to many people.

Again, thank you very much for giving me the chance to publish my cold fusion book.

Very sincerely yours,
Tadahiko Mizuno
Division of Quantum Energy Engineering, Research Group of Nuclear System Engineering, Laboratory of Nuclear Material System
Faculty of Engineering, Hokkaido University

❄ ❄ ❄

To the Editor:

I just purchased Jeane Manning's book *The Coming Energy Revolution* and found *Infinite Energy's* address. Enclosed is $5 for a sample issue of your magazine.

Admiral John Shanahan of the Center for Defense Information says, "Conservation and the development of alternative energy sources in the United States will be a far greater blow to Saddam than the tomahawk missile." He is so right.

United States oil supplies will be virtually exhausted by the year 2020. At that time, the United States will be almost 100% dependent on foreign imported oil and most of that oil will come from the Middle East.

Anybody with common sense can see it will never happen. Somewhere along the way, the price of petroleum will skyrocket and the results will be a worldwide economic and industrial collapse. We could see high unemployment here in America with long lines for the basic necessities of life, bank failures and political instability. We could lose many of the freedoms we enjoy today.

To keep the peace, we need clean renewable fuel—energy that's available forever. We need a "Declaration of Energy Independence" from foreign-imported oil. Let's work to protect our economic security. Let's give ourselves and our children a future beyond oil.

Donald Dyall
Mt. Pleasant, Iowa

❄ ❄ ❄

The following letter was sent to us from *Analog* Magazine.

Dear Analog:

In your July/August 1997 issue, there was an article by Dr. Eugene F. Mallove titled, "Cold Fusion: The 'Miracle' is No Mistake."

In said article, Dr. Mallove mentions a few companies involved in cold fusion research and, more importantly, commercial applications. They are, therefore, apparently pioneers in their field and I can definitely appreciate and respect that because my uncle, Lt. Col. Lawrence Campbell, Jr., was the first African-American to pilot (fly) a jet fighterplane and the first African-American to command an Air National Guard unit.

After reading Dr. Mallove's article, I became convinced that cold fusion and the devices, technologies and applications supporting, deriving from and related to it, really are in the same state of relative position as that of, as Dr. Mallove said, heavier-than-air powered flight at the beginning of this century and we all know how well and how far the aeronautical and aerospace industries have progressed since then.

That being the case, could you please mail to me the addresses of the following companies that Dr. Mallove mentioned in his article, because I am quite interested in finding out the financial possibilities that might exist with them, such as investment opportunities, stock that they are offering and if they will need salesmen and/or dealerships for their revolutionary technologies: ENECO of Salt Lake City, Utah; Clean Energy Technologies, Inc. (CETI) of Sarasota, Florida; Hydro-Catalysis Power Corporation of Malvern, Pennsylvania and Labofex of Concord, Ontario, Canada. Thank you.

Richard C. Campbell, Jr.
Portland, Oregon

❄ ❄ ❄

The following letters concern the paper in *Infinite Energy* Issue No. 21, "A Possible Scenario for the Onset of Cold Fusion in Deuterated Metals" by R. H. Parmenter

Dear Bob [Dr. Robert Bass]:

I have only recently had the opportunity to study your critique of my paper in the latest Infinite Energy (No. 21) magazine. Please excuse the delay. On page 45, you raise the issue of the justification for Equation (32). I believe you yourself have supplied the correct justification on page 48. To understand the inequality (33), note that it implies that the portion of the wavefunction inside the barrier is much smaller than that portion outside. As long as the inequality holds, one can ignore as negligible the process of tunneling from inside the barrier back to the outside, as is implied in deriving (32). If (33) were an equality, I doubt if experimenters would survive the onset of excess heat.

I also agree with your comments about Peebles on page 48. With regard to the discussion of cold fusion in his book on quantum mechanics, Peebles fails to consider a crucial aspect of the problem. Let me quote from the final paragraph of the third paper of Parmenter/Lamb. "An important part of our treatment is the recognition that the barrier to be penetrated by quantum mechanical tunneling is that associated with a pair of composite particles, each particle being a bare deuteron plus an associated screening cloud of electrons." The barrier to be penetrated is not a bare Coulomb one.

Best regards,
Bob Parmenter

Dear Bob [Dr. Robert Parmenter],

Thanks for explaining the meaning of (33), which now makes good physical sense (in terms of the kind of resonant-transmission argument which I learned from Prof. Barry Merriman and rehearsed in my critique), and I hope will convince Barry also of the soundness of your analysis. Also I hope that Prof. Peebles will now consider carefully your point (from the third Parmenter-Lamb paper) that the barrier to be penetrated is "not a bare Coulomb barrier" but that of deuterons each of which has an associated screening cloud of electrons; indeed that is the aspect of your work which I found most enlightening and was happiest to learn (and add to my own Coulomb potential as a harmonic restoring potential). In this connection I note that the new Infinite Energy Press book *Nuclear Transmutation: The Reality of Cold*

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Fusion, by Tadihiko Mizuno [transl. Jed Rothwell] quotes an orthodox QM theory that includes electron clouds and a harmonic restoring potential as possibly explaining a hypothetical beta-capture proton-transmutation reaction that could account for the convincingly-observed, variegated low-energy-induced transmutations of the metallic ions of the electrode itself. I agree with Mizuno that what is now being discovered by the non-dogmatic minority is not a mere island, but a new continent!

Regards,
Bob Bass

the following are comments from our online guest book and www information site. http://www.infinite-energy.com

"Lots of people think that cold fusion is impossible. There's more technology than science in cold fusion and nothing but perpetual engines is impossible. Cold fusion is not PE, so it is guaranteed." Roman

"It's good to know that you exist and that cold fusion is still alive."
Montford Smith

"Very good source of information and ideas that are accessible to someone like me who has some background scientific knowledge but does not have any particular training in science."
Kerry Hand

"I've noted that your publication evades the input of skeptics. If you defend a fringe position, you should address concerns by sincere skeptics."
Pharlos

"I would like to see results from participating labs made public online for purposes of peer review and experimental verification." Colin Sheaff

"I'm doing a report about cold fusion and this page helped me out a lot. This page is cool." Wes

"The time will come when a cold fusion device will power every home independently. But, it will take another Bill Gates to do it." Herman Kurth

"I just love science and your site."
George Vierling,
14 years old

Editorial continued from page 5

occurs at CICOP.

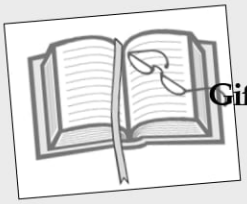
Another wonder of the Cold Fusion and New Energy Age will be the collapse of some of CSICOP's religious fervor as one of its most significant failings becomes apparent to all. CSICOP has deceitfully tried to pass itself off as as an organization dedicated to scientific inquiry in the matter of anomalous energy research. Its own words and actions confirm that at least in the matter of cold fusion energy, some of CSICOP's major figures have been caught at intellectual fraud. In this case, the fraud is not a small matter or a case of differing philosophical viewpoint about matters of the spirit. This is propaganda and fraud on an unsuspecting public that has affected the very pace of life-sustaining technologies. Fortunately, it is CSICOP, not we, who are going to lose the Cold Fusion War.

Published by the

Fusion Information Center
3084 East 3300 South
Salt Lake City, UT 84109

A Quarterly Journal
Hal Fox, Editor

Subscription:
$150 for 4 issues
Single Issues: $45



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BUREAUCRACY IN ACTION

1. Start with a cage containing five apes. In the cage, hang a banana on a string and put stairs under it. Before long, an ape will go to the stairs and start to climb towards the banana.

2. As soon as he touches the stairs, spray all of the apes with cold water. After awhile, another ape makes an attempt with the same result—all the apes are sprayed with cold water.

3. Turn off the cold water. If, later, an ape tries to climb the stairs, the other apes will prevent it even though no water sprays them.

4. Now, remove one ape from the cage and replace it with a new one. The new ape sees the banana and wants to climb the stairs. To his horror, all of the other apes attack him. After another attempt and attack, he knows that if he tries to climb the stairs, he will be assaulted.

5. Next, remove another of the original five apes and replace it with a new one. The newcomer also goes to the stairs and is attacked. The previous newcomer takes part in the punishment with enthusiasm.

6. Again, replace a third original ape with a new one. The new one makes it to the stairs and is attacked as well. Two of the other four apes that beat him have no idea why they were not permitted to climb the stairs, or why they are participating in the beating of the newest ape.

7. After replacing the fourth and fifth original apes, all the apes which have been sprayed with cold water have been replaced.

Nevertheless, no ape ever again approaches the stairs. Why not?

"BECAUSE that's the way it's always been done around here."

(Circulating on the Internet)

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Progress in Catalytic Fusion

Birth of a Revolution in Cold Fusion?

To the delight of many at the Seventh International Conference on Cold Fusion (ICCF-7) in Vancouver, BC last April, a new approach to cold fusion emerged. Dr. Les Case, an experienced chemical engineer with four degrees from MIT, announced what he is calling "catalytic fusion"—to distinguish it somewhat from the original electrochemical approach. He had concluded that the electrochemical method of Fleischmann and Pons was going to continue to be limited by materials issues—palladium cracking, composition, etc.—and the inherent difficulties of working with electrochemical systems. Furthermore, he wanted to achieve the higher temperatures that are allowed by gas-phase systems.

The story of Dr. Case's discovery of catalytic fusion is exciting, including his travel to Europe and Japan in search of the proper path forward. In the account below, we let Dr. Case tell the Edisonian story of discovery in his own words. It turns out that relatively simple catalysts—off-the-shelf "hydrogenation" catalysts used in the chemical industry— seem to catalyze deuterium (heavy hydrogen) gas to helium-4 in a heat-releasing nuclear reaction that is millions of times more energetic than any conceivable chemical reaction. These catalysts are typically activated carbon that has been doped with precious metals such as palladium. Other catalysts may emerge as a result of this line of investigation, ones that perhaps will not require any precious metals. Unlike high temperature plasma fusion (hot fusion), there is no harmful radiation from the process. Thus, the original promise of cold fusion may now be realized in more robust and repeatable experiments. Ultimately, these could be commercialized in relatively straightforward ways that make use of chemical engineering practice.

At the moment, catalytic fusion studies are proceeding at Dr. Case's own lab in New Hampshire, at SRI International in Menlo Park, California and at the Pacific Northwest Laboratory, (a U.S. Department of Energy lab, under contract with Russ George's Saturna Technologies, Inc.). In our own facility (New Energy Research Lab—NERL) here in Bow, New Hampshire, we saw the positive results of a Case experiment first hand shortly after ICCF-7 (see *IE* No. 19). We are beginning a second round of work to demonstrate the process with a relatively simple calorimetric dewar set up. We hope that these efforts help catalyze new work by others in an area of immense potential.

We are pleased to present the following progress reports on catalytic fusion, in the words of Dr. Case and Dr. Michael McKubre.—*EFM*

EDITOR's NOTE: In the course of video-taping our forthcoming documentary about cold fusion (*Cold Fusion: Fire from Water*), our video team visited Dr. Les Case this fall in his basement laboratory in New Hampshire. These are some of Dr. Case's recollections about his discovery and his projections about the future of catalytic fusion technology. —*EFM*

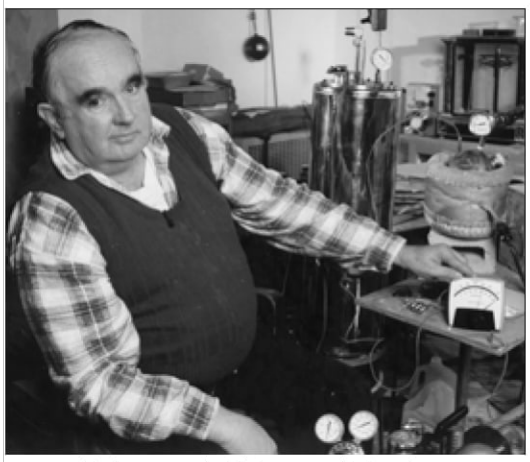
How I Discovered Catalytic Fusion

Prologue

I was going to be a chemical engineer and then head a large corporation. I went to MIT and I got three degrees in Chemical Engineering through the Sc.D. Also, along the way, I took a side degree in Business Administration. I went to DuPont to their Central Research Station, the Plastics Department, or something of the sort. I worked there and it became clear that they didn't want to do business the same way I wanted to do business, so then I taught school for ten years.

I started my own laboratory, studying improved plastics and polymers and I had, for fifteen or twenty years in Nashua, New Hampshire, my own company and my own building, but it never went commercial. I did a lot of research and development, got a lot of patents, and then my wife got very ill. I spent a fair amount of time concentrating on keeping her well. So the laboratory there went inactive. Then when my wife died in 1987, I had a lot of things to do to get the estate together and so forth. I was then following scientific developments, which were then current. I became quite interested in high temperature superconductivity. In fact, I went to the Beijing Conference on Rare Earths and presented a theoretical paper providing the background, what I thought was the chemical background for the physical phenomenon of high temperature superconductivity. For a while I began to play around with the idea of getting a useful device based on high temperature superconductivity.

At just about that time, the cold fusion hubbub erupted. I followed it with some interest, but I could not see how it would go commercial. The original conception obviously was a scientific curiosity, but it wasn't at any point in the reasonable future heading towards a commercial operation. So I followed that at arms length until I saw some work by Dr. Yamaguchi at NTT in Japan, in which he had obtained an 800°C-plus exotherm [exothermic reaction] with, he thought, big bursts of neutrons. So I went to visit him—actually in Tokyo at his laboratory—and looked at his equipment. Beautiful stuff! Very careful work. Clearly, he had obtained a result which was very, very definite. And, incidentally, at this time, which was about 1993 or so, it was



Dr. Les Case in his basement laboratory.
Photo: Free Spirit Productions

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nite. And, incidentally, at this time, which was about 1993 or so, it was still highly controversial as to whether or not anything related to cold fusion had ever really been seen in a definitive fashion. There was no question that he had seen a very definitive result. He'd obtained 800°C-plus.

Well then, I thought, "OK, this is something that needs to be refined and scaled up." And because he was working with palladium and everybody else was working with palladium and platinum primarily, it became sort of obvious to me that probably some sort of catalytic effect was involved. I am a chemical engineer and chemical engineers use chemical catalysis all the time. Platinum and palladium are the preferred catalytic metals. So I then embarked on trying to follow up Dr. Yamaguchi's work in my own fashion. I was initially concentrating on the neutrons as being something important. I then spent, I think, over a year trying to find a laboratory, equipped to deal with neutrons, which would cooperate with me—in which I could sponsor some work and try to work out my ideas.

Off to Europe

There was no laboratory in the United States that I could find that would work with me. After all, it was cold fusion, or something related to cold fusion and most scientists wouldn't touch it—even for money. I finally determined that because all Eastern Europe is known to be very low wage scale—low price scale—that there were some Eastern European neutron laboratories that were of possible interest. So I got myself a plane ticket to Berlin and took the train going east to Warsaw.

I went to the Department of Nuclear Science or something of the sort in the Physics Department in the University of Warsaw. I met a nice lady there and there was a possibility of doing some work. We agreed to meet a little later on my trip to Budapest, for dinner and further consultation. Then I looked at the train schedule and considered going to Lotz. It's not very approachable, so I skipped directly to Prague, which was a lucky shot. I went to Prague and I knew about Charles University there, which is a very famous old university, and went downtown to the old town square to the main campus and tried to find the Physics Department. It wasn't easy, because I don't speak Czech and many of the Czechs don't speak English. I finally found somebody there and she told me, "Oh, you want to go the Physics Department. That's on the other campus, across the river."

So I got the directions to go to the other campus. It's a tower building there and the Department of Nuclear Science was on, I think, the tenth floor of this tower building. So I had the taxi driver let me off and I went to the tower building, found the elevator, and went up to the 10th floor. I walked out the door and there was a sign that said "Nuclear Science." I went in and there was a very efficient scientific looking gentleman with white hair, sitting there talking to, I guess, the secretary. It turned out he was the Director of the operation.

I explained to him I wanted to do this kind of research and he said: "We'll do it!" I said "Really, who has to approve it?" and he said: "We'll do it!" So I hooked up with the Department; actually it's the Nuclear Center, Department of Physics and Mathematics at Charles University. For I guess over a year, maybe about two years, I was doing experiments in their nuclear laboratory, which is associated with CERN. It's a serious nuclear laboratory. It is by no means equivalent of CERN. . .

Catalytic fusion reactor in Dr. Les Case's lab, showing pressure gage and resistance heater collar. Photo: Ed Wall.

Shooting in the Dark

It was empirical work and I was trying to find an effect—the idea was to find some sort of temperature [rise]. I was using the temperature gradient for a catalyst—active versus a blank. I had a big vessel, and I had four samples inside the big vessel. One of these four samples was the blank and the other three were potential candidates. I would change the hydrogen or the deuterium gas over the sample, change the nature of the samples, and look for temperature differences. With neutrons or without neutrons. We also had to measure the neutrons I might be making, so it was empirical. I made a whole bunch of runs, —oh, on probably three or four different trips, and with minimal results for maybe the first two trips. One of the times I started with a plated palladium-on-copper tubing, and I thought that might be catalytic, but it wasn't. I tried some titanium tubing, but it wasn't catalytic, and I finally ended up thinking: "If it's catalytic, you better use catalysts." So I ended up scanning through several dozen available samples of catalysts.

Finally, some of these catalysts I was modifying—I actually had some platinum and palladium acetonate, and I was modifying the surfaces—all of a sudden we started seeing temperature differences in one or two of the samples. That is, we were beginning to find active catalysts that would really show a temperature gradient over the inactive catalysts. And I can remember very clearly, one day it was, I think 1.2°C or 2.1°C above the background in a particular catalyst sample. The physicist that was working with me was amazed, because as far as physicists are concerned, 1 or 2°C might as well be a million degrees, because it's clearly an effect and we were measuring it immediately versus an adjacent blank.

He said. "Well, how did you select this material to do this experiment?" And I said: "Because that's the one that works!" This is what happened: I had scanned through with many different experiments through all the various candidates that I had received from three to five different sources of catalyst, until I found a catalyst, a chemical catalyst that was off the shelf, that actually worked to give some sort of effect with deuterium compared to hydrogen and compared to the other blanks. So it was strictly an empirical result, just blindly following my nose. Changing the conditions, changing the pressures, changing the temperatures, and so forth until I finally found a catalyst that gave me a result. . .

What happened was as follows. I have always been very protective of this. Well, not always, but for the last five years or so—very protective of the results—not disclosing them to anybody. I have a series of U.S. patent applications, about eight or ten of them, a basic one which was totally speculative and wrong. I kept filing continuations and amendments to them. Finally, I began to get these results, and then with all of our three or four patent applications prior to my current ones, I

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applications prior to my current ones, I began to get results. I kept improving them.

Finally, I got to a set of results which defined the field, basically. With that patent application, I filed for foreign applications and that was published in November 1996. I expected that there would be a very big response when this was published, but there was no response whatever. Nobody was paying any attention. So finally I decided to take the bull by the horns and I appeared at the Cold Fusion Conference unannounced, in Vancouver in April of this year. At this April Cold Fusion Conference, ICCF-7, I gave a brief talk, saying that I had developed an experimental procedure for reproducibly generating a heat effect with deuterium and that it's catalytic. As I say, I can reproduce it and I can scale it up. It created quite a stir at the conference, because people were looking. A lot of people were looking for this: some sort of basic real approach, not just playing around, but a concept of something that made it work reproducibly. The concept I introduced was contacting a certain limited range of standard chemical catalysts with deuterium under standard conditions, and it would work.

Well, there's a little bit more to it than that, but this was new because nobody previously had ever used a standard chemical catalyst. They were always making their own special material and practically nobody thought of a catalyst. It was their particular equipment, and sometimes it was very elaborate. But I was able to buy, off the shelf, standard chemical catalysts which did work. Gene Mallove and I met at that conference. This is how Gene and I came together at the conference in Vancouver.

Latest Experiments

Well the situation basically is this. This is the vessel. It's a modified oxygen tank and in it is a thermo-well, this is a gas inlet and outlet, and this is simply a port for putting solids in or out. Now in the bottom of this vessel, which is heated in this jacket, there are about 40-50 grams of standard chemical catalyst. It's been contacted now with deuterium gas for six or seven weeks and, using hydrogen in this vessel under exactly these conditions, I got a steady state temperature of 181.5°C. Now, when I switched to deuterium it started off about 180°C, slowly rose over the space of two or three days, and finally levelled out at about 220°C, maybe a little bit more than 220°C. Right now it's about 215°C, almost 35°C hotter with deuterium inside than it was with hydrogen. This is excess heat, which is apparently occurring due to deuterium fusing to helium-4.

So, inside this vessel now for six or seven weeks, we have had deuterium fusing to helium-4 and giving this excess temperature of about 35°C, which is big—a really big effect compared to previous effects of practically unmeasurable temperature increases. This one is now continuing and maybe will continue for some weeks or months still. The idea is to test the reliability of the catalyst. The catalyst must work for some months or it's not a viable commercial process. You have to be able to load up your reactor and have it generate the heat for months without having to re-do the catalyst, because it's expensive and too much of a problem. So this is rather encouraging. It looks like it may be totally stable, or at worst, over the space of many months drop 10, 20, 30% in activity, which is acceptable.

Dr. Case's large modified dewar cell, designed for catalyst beds up to one kilogram and aimed to achieve self-sustaining. Deuterium gas leaks are being fixed. Photo: Ed Wall.

Helium Measurements

Now, when this experiment is concluded for one reason or another, a gas sample is going to be taken off through here and analyzed for helium-4. With any luck, it may even read over 100 ppm of helium-4, maybe 200 or 150 parts per million. It won't be going up to a thousand parts but it's going to 50 or 100 or more. This is very very significant, because the helium-4 content of air is 5.2 ppm. So anytime you get above 5.2 ppm you're *making* it. So this vessel is sitting here making, as we watch, helium-4 at a temperature of 215°C. Now this is a very novel concept: that you can have nuclear fusion occur at 215°C and one atmosphere pressure. Those are very, very mild conditions compared to what they're doing in plasma fusion and the H-bomb.

I had run this experiment several times before and obtained samples which I had analyzed at the OakRidge National Laboratory by the kind people at Lockheed Martin. I had some trouble with leakage and sent some bad samples and one or two fairly decent samples. One sample was contaminated after I adjusted the leakage and measured something like 100 ppm of helium-4. But they were able to analyze a good sample at something like 91 ppm of helium-4. Now the equipment is not ideal, because it's a big magnetic sector instrument and it separates out helium-4 from deuterium, which also has a mass of 4 by a very small difference in mass—something like 1%. That's the only way they do it, they don't trap out the deuterium. Because the helium is at a very low concentration, they see the helium-4 peak as just a bump on the side of the deuterium peak. So it's very iffy.

Now, some of the people at Vancouver [ICCF-7], at least, saw this as not particularly reliable, but certainly interesting. They began to try to reproduce this rather quickly in May. Certainly by June other people were trying to reproduce this result. One of the people who tried to reproduce it was a man named Russ George who has an association with SRI International in Menlo Park, California. He set up their equipment, apparently with permission of the group, and tried to reproduce this. The way he originally set it up, it didn't work. He got no [excess] heat and, of course, no helium. We had a brief consultation about it and I explained to him that you can't run the apparatus that way. I made a couple of suggested changes and it immediately took off with heat generation. Then he used their mass spectrometer instrument to analyze for the helium produced after 24 or 28 days, and he got a helium content up to about 11 ppm, which is far above anything that can be explained from leakage in from the air. And, because it had started at zero and went up to 11 parts per million in a monotonic way, that is, always a rising function, it clearly was coming from inside the vessel and not from contamination.

Now, those data aren't considered by the people at SRI to be definitive enough to be published. They are very, very strongly indicative that there is helium-4 generation by this fusion under these conditions. Now that result is going to be re-confirmed by SRI in a much more

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careful and definitive fashion. When the data are finally very very firm and unassailable, "bullet-proof," they call it, that will be published in a definitive paper saying this is now proof that we are getting helium-4 generated and we get a correlation between the helium-4 generation and the heat output. This clearly is a catalytic fusion, it really is working and, in fact, it is a new branch of physics.

Small Case catalytic fusion cell (inside glass dewar, center) set-up for calorimetry calibration and "live" operation. Note: gas cylinder safety cap is used to secure thermal cover on dewar. NERL, Bow, New Hampshire. Photo: Ed Wall.

Scale-Up

My objective always has been not to play around scientifically, because I'm not really a physicist, but to head towards commercialization. I really want to go to a 100-megawatt reactor within two to three years, which is really compressing the time scale, but it may be possible. So the idea is to scale it up. Now I wanted to scale it up, but other people want me to have it so it can sit there and, for instance, unplug this electric heater and it stays hot— self-sustaining heat or, as Gene Mallove says, "Life [sic] after death" [heat after death]. It will stay hot without any heat input from the outside.

Well, I'm trying to achieve both a scale-up and self-sustaining heating by bringing it up to a larger scale. This one has 40 grams of catalyst in it. This is a much larger vessel, this happens to be a modified stainless steel dewar, which is an insulating vessel. In this I will have one kilogram of catalyst, which is 25 times as much as in here. But the heat loss is not 25 times as much as the bigger vessel. The heat loss is maybe three or four times what the smaller vessel has. So if I had three or four times this heat loss and 25 times the heat generation, then presumably this one might self-sustain.

Maybe I'll get 250, approximately 250 watts of heat output from the catalyst inside this larger vessel. So this is a model scale up of the same reaction in this flask. The stainless dewar is as it came from a cryogenics apparatus. This is the cover and these are steam tubes. This is a heating device. The heat comes into this immersion heater, which is transferred to this aluminum fillet, which is transferred through this inner tube. I call this a "hot finger," the heat is being transferred into the hot finger and then it goes into the deuterium gas. If necessary, I will take some heat out using the steam tubes. There's a pressure gauge here and a gas inlet and outlet. I have two thermo-wells. I can use a thermocouple and stick it into either of these two thermo-wells. One of the thermo-wells is dipping into the catalyst layer, the other is out in the gas phase. However, it isn't that easily constructed. Inside there are some tricks to the way it's been defined and the way it's going to run. But the hope is that this, which will be run within a few days—I finally got it ready to go, work in progress, you know. Within a few days it may reach self-sustaining heating. And then, of course, the idea is: OK, so this is 250 watts, now let's go to 5 kilowatts. Once I go to 5 kW then I'm going to ask someone for some money to design 5 megawatts, or something of the sort.

It is critical the way you have the gas in contact with the catalyst, that's clear. That's been shown by the previous experimenters. With careful scale-up and changing the way the thing is done there's no reason why it can't go to 25 megawatts and 100 and then maybe 1,000 megawatts. I'm going to stop there. A thousand megawatts—that's big enough.

Implications

There are very many implications of this for society. One of them is that there's enough deuterium in the oceans to satisfy all the world's energy needs for a hundred million years. So there's more potential energy in the deuterium in the oceans than there is in all the fossil fuels combined by a factor of, what, a million or something, maybe ten million. But that isn't all. It isn't just that there's an unlimited supply of future energy. This is very cheap energy, because deuterium from the oceans compared to the amount of energy it produces is very, very cheap. The fuel cost is very much lower than fossil fuel. Deuterium as a fuel is surprisingly much cheaper than coal, and this is a big shock to people to contemplate an energy source much, much cheaper than coal. As a matter of fact, it may be more than two orders-of-magnitude cheaper than coal.

That isn't the end of it. The byproduct or, rather the product, of this reaction is helium-4, that's pretty clear. Helium-4 is totally inactive and benign. If you want to you can vent it to the atmosphere. It doesn't make a bit of difference. So this has the promise of getting rid of the greenhouse effect [threat]. When you substitute deuterium fusion for fossil fuel combustion, you start cutting down to the extent that you do that substitution. You cut down on air pollution, you cut down on the greenhouse effect, you cut down global warming. So, ultimately, in ten years or so, we will have totally defeated the greenhouse effect and global warming and air pollution—all at the same time. The public needs to really understand that. It's critical to develop this as quickly as possible to cut down on these horrendous problems of global warming, the greenhouse effect, and air pollution.

Dispersed Power Generation

It is going to be possible, I believe, to design a passive non-moving source to maybe 5 kilowatts or 10 kilowatts, using the technology represented by this, assuming that it works. But it's not going to be possible to scale up to megawatts. It's going to be possible to go to a few kilowatts. Now a few kilowatts is sufficient for a house, and it would make steam and electricity at the same time using a small co-generation unit, or it could be made slightly larger for an apartment or for a location such as a mountain top villa or something of that order. But I cannot conceive of scaling this up, this type of technology, to megawatts. So there will have to be a fundamental redesign of the reactor. I have some strong ideas on how that should be done. Also, you are going to have to change the catalyst. This depends on palladium or platinum metal. There is a very definite limitation on the amount of palladium and platinum metal that's available for the world. If you were to use palladium catalysts of the type that's now in sight to built a 100 megawatt plant as a small commercial-sized power plant, you need something like 5% of the world's palladium supply in one power plant. You can't build very many power plants a year without severely impacting the palladium market. So there will have to be a change of the catalyst.

I have some far-distant ideas on that. So there will have to be a way to use titanium or nickel or some other metal—a non-platinum group metal as the catalyst—as one scales up and goes

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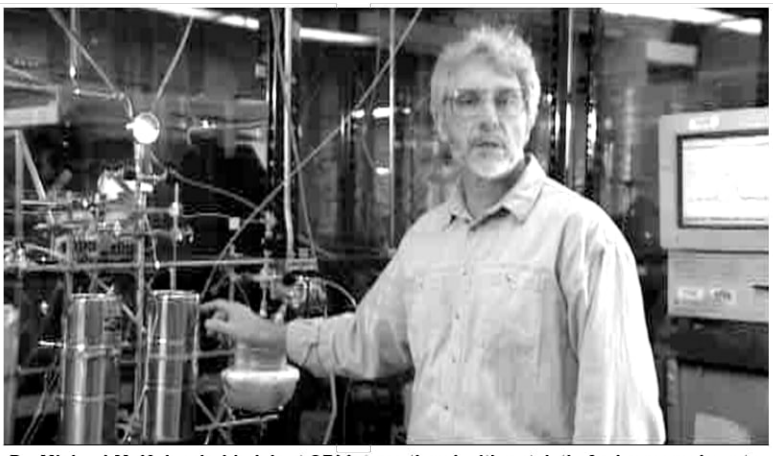
commercial. That may take some years, but that clearly is the way for the future.

This is the key to the whole thing. I discovered that using certain standard commercial catalysts, one could get this fusion to occur under reproducible, mild conditions. This is the catalyst that I've set upon as being about the most effective that I currently have available. This is a standard palladium on activated carbon catalyst. One-half percent by weight of palladium loaded on this activated carbon—this is the key. You change this just a little bit and it doesn't work—at all! But if you stay within the approved ranges, it works basically all the time. This is my contribution to find that that specific catalyst, within a certain limited range, operates under these standard conditions.



Dr. Michael McKubre in his lab at SRI International with catalytic fusion experiments. Photo: Free Spirit Productions

EDITOR's NOTE: In the course of video-taping our forthcoming documentary about cold fusion (*Cold Fusion: Fire from Water*), our video team visited Dr. Michael McKubre this fall in his laboratory at SRI International in Menlo Park, California. These are some of his comments about the status of his group's experiments to verify the work of Dr. Les Case in the United States and Drs. Arata-and Zhang in Japan (see *IE* Issue No. 18 for Mike Carrell's summary of the latter). Though understated and cautious, as befits one of the field's foremost scientists, it is clear from what Dr. Mckubre says that much progress is being made.—*EFM*

COMMENTS BY DR. MICHAEL MCKUBRE

The experimental apparatus here is really set up to see whether or not helium can be produced by exposing a carbon catalyst with palladium to deuterium at slightly elevated temperatures and slightly elevated pressures.

This experiment very much follows along the thought process of Les Case and behind me you see five different sets of apparatus. The big vessel here is one of Les Case's, he calls them "footballs," it's a stainless steel vessel—on a heating mantle set up in exactly the arrangement that Les Case himself is doing in New Hampshire.

What we have behind me are four different generations of the Case experiment. There's the original Case experiment in this "football," as he describes it—a cylindrical stainless steel vessel on a heating mantle, a very simple experiment in which you simply put deuterium gas in and monitor for helium production. The first attempt that we had at SRI was formed in these vessels we called "Vessel 1" and "Vessel 2," slightly more sophisticated vessels which you can't see. They are concealed in the stainless steel dewars for heat retention purposes. Originally we had Vessel 1 filled with hydrogen and Vessel 2 filled with deuterium, so we could see whether the helium we were observing was present in the deuterium cell or the hydrogen cell. As it happened this cell Vessel 2 produced something like 11 ppm of helium. Vessel 1 at no stage produced any helium, suggesting that our helium determination process and our leak-tightness was, in fact, satisfactory for this experiment.

The original experiment in Vessel 2, as I said, produced 11 ppm helium. The air that we are breathing in this laboratory now is 5.22 ppm helium, so there is very little opportunity for error. The helium in the vessel, apparently, was produced by some source within the vessel and did not come from the air that we're breathing.

We're running now a second generation of this experiment in these two vessels. It's early stages yet, but we're in the hopeful that we'll be able to reproduce our own result which was, of course, a replication of Les Case's result.

This is a more sophisticated experiment. The question is, does the movement of the deuterium gas play any role in the production of helium. Is convection an issue? Is temperature gradient an issue? In this experiment, which, again, is concealed inside this dewar flask and non-observable, we're simply recirculating deuterium gas over a bed of Les Case's catalyst in a continuous manner and sampling periodically for helium in the deuterium gas. Behind the bullet-proof [transparent] polycarbonate wall here is a high pressure experiment, and this is our most recent attempt to see what the parameter space is for the production of helium from deuterium and carbon catalyst. What is the pressure effect? What is the temperature effect?

Les Case has already explored the temperature dependence somewhat. He finds that the effect occurs in a range of 170°C up to about 270 °C. We have not explored the temperature domain, and until we get a lot more apparatus we won't do so. But we are able to explore the pressure domain somewhat better than Les Case is able to do because we have somewhat more sophisticated apparatus.

In the vessel on the floor, we have a high pressure deuterium gas at intermediate temperature about 200°C. This experiment, in fact, just started about two days ago. We have no reason to expect helium production as yet, and the analysis reveals none so far.

All of these experiments are connected to a common gas manifold. What we are able to do is take a sample of the gas from each of these cells periodically. Initially we did it daily, but now we are doing it every two days, in fact three times a week, so we submit a sample of gas from each of the cells for analysis to the mass spectrometer, a high-resolving, low-mass mass spectrometer. We're capable of separating the two masses of species, deuterium D_2 and helium-4. The sole purpose of this experiment, the sole purpose of this apparatus, is to measure helium-4 in the presence of deuterium D_2.

On the monitor you see displayed, in fact, the mass spectrum from one of these samples. This is a relatively high level of helium-4. The peak here is the helium-4 peak, the deuterium peak would normally appear here; it's completely absent. This particular example shows 10.5 ppm helium. We compare the samples each day that we perform the analysis, we compare the samples of gas from the various active cells and blanks with a sample of room air, which we have measured many, many times and know to be 5.22 ppm. And we have some standards, which we typically use—that is, gas samples of helium in deuterium and argon which we submit to the mass spectrometer for the purpose of calibration.

The mass spectrometer simply sweeps a mass from low mass to high mass, in this case from 3.96 mass units to 4.06 mass units, which encompasses the range in which helium is to be found. In fact, this peak is helium, and deuterium D_2 is to be

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found which will be found somewhere in this region. We use a liquid nitrogen cooled carbon trap in order to remove D_2 so that we're able to see quite low levels of helium. We're accurate to probably 0.1 ppm helium and we can clearly resolve the presence of deuterium D_2 and helium-4. This spectrum is, in fact, the sum of a number of spectra that the mass spectrometer simply sweeps for the period of time that we pre-program, and this is the cumulative signal representing the integral of all helium which was present in the sample when we submitted it for analysis. To acquire this spectrum takes us about five minutes.

It's clearly not possible to produce helium from a chemical process. If we observe helium in our experiments it's either because it leaked in from the atmosphere—we can rule that out by the blanks that we do and the fact that the helium signal that we have seen is larger than the helium in the ambient. It's possible that the helium pre-existed in the sample and was simply released to the gas phase with long term exposure. We can rule that out largely because we've analyzed the catalyst that we're using and found that it contains no measurable levels of helium.

The only possibility that remains, and remains to be checked, is that the helium is produced by a nuclear process. If the helium is produced by a nuclear process, then necessarily there will be an associated release of heat. Although these experiments were not initially set up to be rigorous calorimeters, we have monitored them with a sufficient number of temperature sensors that we can know, to some degree with some confidence, whether or not heat is being produced and at what time heat is being produced.

From the best of my ability to analyze the thermal record, it appears that, yes indeed, in the vessel that was producing helium there was some evidence of excess heat and that the amount of heat produced was approximately quantitatively correlated, that is, the right amount of heat was produced compared to that of a nuclear process involving deuteron-plus-deuteron producing one helium-4 nucleus which releases 23.8 meV.

I'd like to re-state that the calorimetry was largely retrospective, this experiment was not set up as a calorimeter and, therefore, the calorimetry is not rigorous, but the temperature record quite clearly indicates in these experiments, as it does in Les Case's experiments, that there is an unexplained source of heat and the magnitude of that source of heat is approximately the right value to account for the observed helium.

Part of this generation of experiments is to improve the calorimetry and the central question in the cold fusion field is: "Is there excess heat?" If "Yes," then, "Is that heat the result of a nuclear process?" So the central question that we're all seeking to answer is: "Is there a quantitative and temporal— is there a quantity-related and time-related correlation between the appearance of anomalous excess heat and the appearance of the product of a nuclear reaction such as helium-4?"

So the thrust of our work is very much to find the heat and quantify it accurately and find the nuclear process and quantify it accurately so we can correlate the appearance of these two products.

View inside dewar showing Case catalytic fusion cell mounted on resistance heater. NERL, Bow, NH.Photo: Ed Wall.

We have determined that there is excess heat and we have to do a better job of measuring it with accuracy. This laboratory here is really set up to do highly accurate calorimetry. That work has largely been associated with the electrochemical experiments, such as Arata's experiments and our own experiments. So we are quite capable and willing to do the calorimetry. We just haven't applied those skills fully yet to the Case experiment, but this is obviously our plan.

One of the difficulties in the cold fusion field is the apparent lack of replicability of experiments: many people performing the same experiment get apparently different results; different experiments performed in the same laboratory give apparently different results. So it's obvious that if you do the same thing you must always get the same result. What this is telling us is that there are some important parameters of our experiments that are not under our control. Some of them I know and understand, and still [we] can't control some of these parameters we don't know about yet. We just don't know what the process is that we are studying, so we don't know what parameters we need to control in order to yield a consistent result.

An experiment which always gives the same result—can be performed in several different laboratories to yield the same result—would be very valuable to us, in part in helping to convince the remaining skeptical scientists in the world that there is a phenomenon to observe. But, in fact, in order to use the scientific method to observe scientific results, we have to be able to reproduce the results of our own experiments so that we can see what the effects of small changes are on these experiments.

The Arata-Zhang Experiment

One experiment which has been reported to produce consistent and reproducible results is that of Professors Arata and Zhang, both of them are very, very experienced and very well recognized scientists in Japan. They performed a very careful experiment, reproduced it apparently a number of times in their own laboratory—producing both anomalous excess heat in fairly significant levels and helium-4 and, perhaps more interestingly, helium-3. The helium-3 to helium-4 ratio that they observed in their experiments is different from that in the air that we're breathing. [Editor's Note: This isotope ratio is off by a *huge* factor—see the Carrell review in *IE* Issue No. 18.—EFM]. Sufficiently different to indicate that there is clearly an anomalous nuclear reaction occurring. The difficulty only with Arata and Zhang's experiment is that it's only been performed by them and only in their laboratory. What we're attempting to do here is to produce their same results with their apparatus and with their help. This is a collaborative effort between Arata and Zhang and the SRI group, to produce in *our* laboratory the same results as they have obtained repeatedly over the years, which would indicate that we have some degree of mastery over the experiment.

The experiment that we have running here, in fact, is relatively young; it hasn't been operating for very long. One of the difficulties with Arata's experiment is that it requires many, many months to produce a

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result, and quite literally we're not very experienced with Arata's methods, so we've had some difficulty getting his experiment set up and operational. Certainly, it's caused me to have an increased level of respect for Arata and Zhang's technical competence. They are very, very good scientists. Within a month or two, we hope to have reproduced their experiment faithfully and reproduced their result. And the benefit will be in part sociological. We will demonstrate that an experiment can be transported from laboratory to laboratory and yield the same result. It will also give us something that we can do again ourselves and define somewhat the parameter space in which these experiments yield excess heat and, apparently, helium-3 and helium-4.

From the best of my ability to analyze the thermal record, it appears that, yes indeed, in the vessel that was producing helium there was some evidence of excess heat and that the amount of heat produced was approximately quantitatively correlated, that is, the right amount of heat was produced compared to that of a nuclear process involving deuteron-plus-deuteron producing one helium-4 nucleus which releases 23.8 meV.

I don't know that Arata and Zhang have monitored their experiments for neutrons. We routinely monitor in this laboratory for neutrons at the radiation hazard level. We have a continuously operated neutron detector for personnel hazards. Clearly, this has not alarmed at any time or I would not be standing here right now. Whenever we've made attempts to look for neutrons in active heat-producing experiments, we have not observed neutrons above background level. That indicates simply that the neutrons, if they are produced, are not produced quantitatively with the heat in the same way that a hot fusion process occurs, but we've never had very sophisticated neutron detection applied to a calorimetric experiment producing large levels of excess heat. The problem is a very simple one, the criterion, the conditions necessary to do a first class calorimetric experiment of an electrochemical process—these conditions are incompatible with those necessary to do a high quality neutron determination. So you either optimize your experiment for the eletrochemistry and calorimetry or you optimize it for the neutron measurement; you can't do both.

In fact, behind me this large black box is a neutron spectrometer designed for us by [the late] Kevin Wolf who, in my view, is the most able nuclear experimentalist that I have ever met. A first class man with a first class talent for low level nuclear determination. It's a beautiful neutron spectrometer and cost us, or EPRI, perhaps $3,000. We've never used it with serious intent because we've never had an experiment which would make it worth our while mounting and manning this apparatus. The neutrons that are present in these experiments, if they are present at all, are present at very low levels. Levels so low that they can't be directly connected to the heat producing process. They may be indirectly connected, but they are not present in large quantities and therefore they are not very interesting to me.

Advantages of the Case Technology

The Case device is attractive for several reasons. It's simply deuterium gas and carbon catalyst—commercial catalyst—something that can be obtained in 55 gallon drums, and the vagaries of the manufacturing process have already been mastered. So that if the Case experiment works to produce heat by a nuclear process, then it's something that can be very easily scaled up. Most of the work that's been done in this laboratory has been done on electrochemical systems which are very sensitive to handling issues, the metallurgy of the palladium, the purity of the electrolyte, and really only people that have been trained for many, many years in electrochemistry are able to perform electrochemical experiments satisfactorily.

In Case's experiment, you have a gas, an easily accessible temperature, modest pressure in a sealed vessel. This is an experiment which many people can do and facilities exist to perform the experiment and understand its sensitivity to the various parameters and it's easily amenable to engineering scale-up.

The big question, of course, if we do have a heat-producing system, if that system requires significant quantities of palladium then its application is necessarily limited. Palladium is a precious metal. In fact, its a by-product of the platinum metals industry. But if palladium were to have a use all on its own, it's price would go up dramatically. It's availability is scarce so that a commercial system based on Case's concept would require a metal other than palladium or a very efficient way of recycling the palladium. We don't know as yet whether other metals produce the same effect in terms of the helium production. Les Case has studied several of the platinum group metals— palladium, ruthenium and the like, platinum and osmium, and has found that the effect is present with most, if not all, of the platinum group metals. This doesn't help much, because they are all precious, so we really need to find non-precious, non-platinum group metal which produces this effect.

My own view is the attempt to scale up is premature. We need to understand the mechanism, the process that we're studying. Once we understand what the mechanism is we will understand what metals or alloys might be satisfactorily used and perhaps optimized; maybe we'll get a larger effect. And only then can we explore the engineering applications.

This is the first Case vessel which we, in fact, obtained from Les Case in exactly the form in which he is performing his experiments in New Hampshire. This vessel, which we call "Vessel 2" and its twin experiment "Vessel 1" are our attempts to do Case's experiment in a similar geometry but a slightly more sophisticated apparatus.

The experiment over here on the left is an attempt to explore whether the convection of gas, that is the recirculation of gas, affects the rate of helium production.

The important parameters of all of these experiments are being recorded by a computerized data acquisition system are displayed on this screen. . . This is an indicator of one of the temperatures being recorded but, in fact, we are recording anything up to 10 or 12 different temperatures in any set of experiments. All of these signals are recorded by computer and we are displaying the most important of them on this computer monitor, that is, the current, voltages and, therefore, powers going into each experiment; the temperatures and the pressures are recorded in each of these experiments. In this present configuration, we are making a measurement every five minutes and recording it to file so that we can analyze it off line to see, for example, whether there is any excess heat, pressure anomalies, leakage and the like.

In an experiment where we are interested in measuring the presence of excess power, we obviously have to record power very accurately and what we use is a Hewlett Packard computer-controlled power supply. Each one of these slots has a different power supply, all of them commanded by the computer to produce either constant current or a constant voltage displayed by the displays here but also recorded by the computer. It's a very stable power supply, very accurate and very constant.

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Non-Stellar Nucleosynthesis:

Transition metal production by DC plasma-discharge electrolysis using carbon electrodes in a non-metallic cell

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Editor's Note: This article is part of our continuing investigation and coverage of the remarkable Ohmori-Mizuno electronuclear class of experiments. We first discussed these in *IE* No. 20. Some of Dr. P. Kanarev's work in Russia, a similar technology that he calls "plasma electrolysis," was reported in *IE* No. 22. Now we have this extraordinary work by Chip Ransford at NRG in Denver, Colorado—*EFM*

Abstract

Described are experiments—simple, but not entirely without hazard—which appear to produce transition metals from lighter elements. The reactions are thought to occur in plasma discharges generated within—and contained by—hot aqueous alkali-metal ionic solutions. These experiments are derived from the recent tungsten-cathode work of Mizuno and Ohmori (ICCF-7) [Editor's Note: See cover story, *Infinite Energy* No. 20]. Ultra-pure, spectrographic-grade carbon rods were utilized as both anode and cathode in all glass and plastic electrochemical cells, with pure electrolytes such as K_2CO_3 and $NaHCO_3$. The cells were run at boiling temperature at atmospheric pressure with 130 to 165 volts DC, drawing 4 to 7 amperes. Brilliant pin-point flashes scintillate over the surface of the cathode during operation, with some concentration of sparking around the bottom edge of the rod. "Sunburns" suffered using exposed cells suggests high intensity UV is emitted. No gamma rays were found from a similar cell. Neutron output is unknown, but moderation/absorption shielding is used as a precaution. One cell produced over 450 mg of metal-rich black precipitates in a run of 20 minutes. Preliminary microscopic elemental analysis reveals inhomogeneous tiny flecks containing chromium, iron, and nickel in quantities well beyond expected contamination levels. Less sophisticated equipment is sufficient for proof, however, since there are enough ferromagnetic materials for their presence to be visibly demonstrated with a strong permanent magnet. A few hundred watts of excess thermal energy was believed to have been observed—active cells tend to produce more condensed steam from the system than a reference heater with the same energy input. Fundamentally puzzling is that much binding energy is apparently missing—tens to hundreds of kilowatt-hours per experiment. There could be an intense neutron flux, or heavier elements may remain to be found (co-produced with Cr, Fe, and Ni in endothermic reactions). Experimental and analytical studies are on-going.

1. Introduction

Under the Big-Bang model of cosmology, an initial event created hydrogen and helium within the first few minutes. Stars later became the nuclear foundries yielding heavier elements from carbon to uranium, according to the theories of stellar nucleosynthesis, first advanced in integrated form in 1957. [See Sidebar on Stellar Nucleosynthesis] (Lithium, beryllium and boron appear to be formed by the spallation of heavier elements by cosmic rays.) These elegantly predictive high-energy theories, however, are very unlikely to have any applicability whatsoever to the results of the present (and similar) low-energy experiments, beyond explaining the origins of the starting materials, and showing the extreme conditions that have been believed necessary for the occurrence of nuclear fusion reactions. Astrophysics has largely ignored the low-energy regime, in both theory and experiment.

Non-Stellar Nucleosynthesis

The study of low energy nuclear reactions (LENR), supported in the former Soviet Union since at least the mid-fifties, has enjoyed a surge of serious interest in the West. This was triggered by the 1989 revelations of cold fusion in heavy water palladium (D_2O-Pd) electrochemical systems by Martin Fleischmann and Stanley Pons, followed by confirmations of tritium by E. Storms, and helium production by M. Miles, and many others. (Not to be forgotten is that Isaac Newton himself practiced alchemy, though what results, if any, were achieved by Newton are unknown.) As if deuterium-to-helium reactions at low energy were not hard enough to accept by the majority of scientists, particularly physicists, these announcements were followed in 1991 by reports by R. Mills of excess heat in light water-alkali metal electrolysis and then evidence of proton capture in finding anomalous isotopic ratios in similar systems (R. Bush and R. Eagleton, R. Notoya). Meanwhile, other researchers were finding inexplicable elements appearing in D_2O-Pd systems (J. Bockris, Matsumoto). The publication in *Fusion Technology* in 1994 of two papers on the production of iron and other elements by underwater arcing between carbon electrodes, brought to light and confirmed work done in the 1970s in Japan by Oshawa. By 1995, sufficient evidence had accumulated for a conference on low energy transmutations to be held at Texas A&M University, with seven experimental reports and several theoretical papers. By the 1996 LENR meeting, over a dozen experimental reports were presented, including important work by H. Fox and the Cincinnati Group, by J. Dash, T. Mizuno, T. Ohmori, K. Shoulders and a pivotal paper by G. Miley and J. Patterson on electrolysis with thin films of metal on non-metallic substrates. Miley's careful analysis showed that in some cases, up to 40% of the parent metal mass had transmuted to other elements. Directly leading to the present work, new ground was broken by Mizuno and Ohmori and presented at the Seventh International Conference on Cold Fusion (ICCF-7) in the spring of 1998. They reported excess heat, neutrons and anomalous elements using metal electrodes in alkali metal ionic solutions in a cell operated at voltages high enough to cause sparking discharges at the cathode. Their work with tungsten cathodes was well covered in *Infinite Energy* Magazine No. 20 and subsequent issues, including a reprint of their paper, along with descriptions of various successful and unsuccessful attempts at replication. The experimental series by this author, started in September 1998 and described in this preliminary report through December 5, 1998, are derived from these sources.

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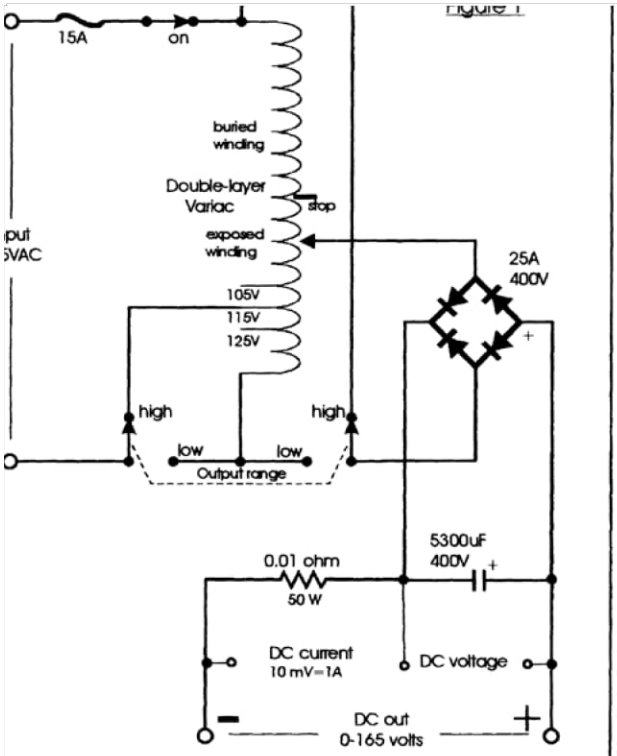
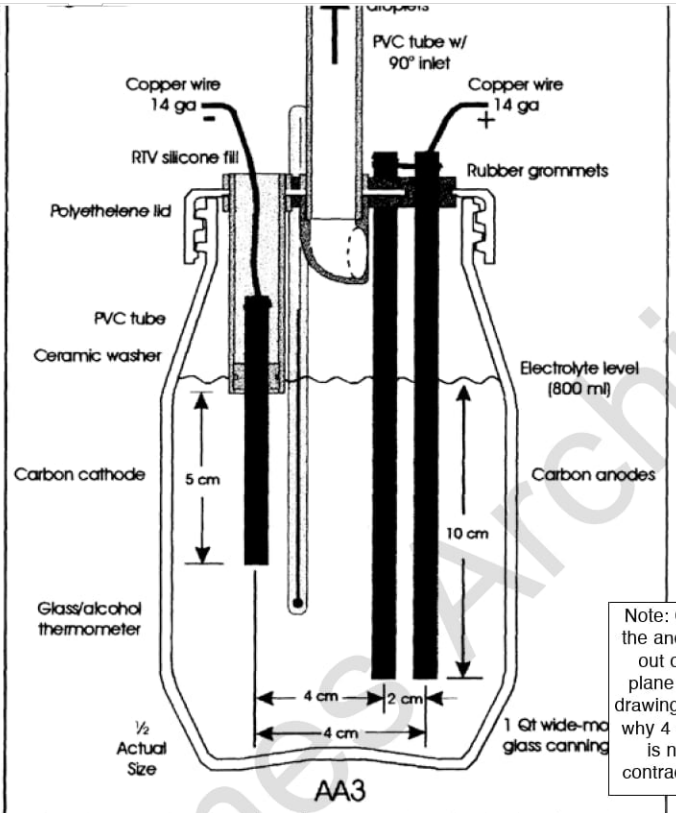
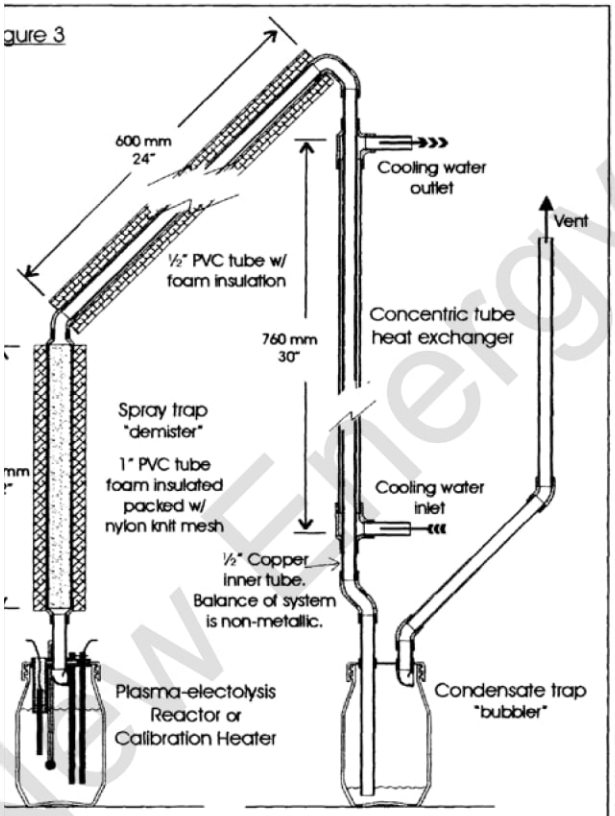


DC Power Supply
One kilowatt output

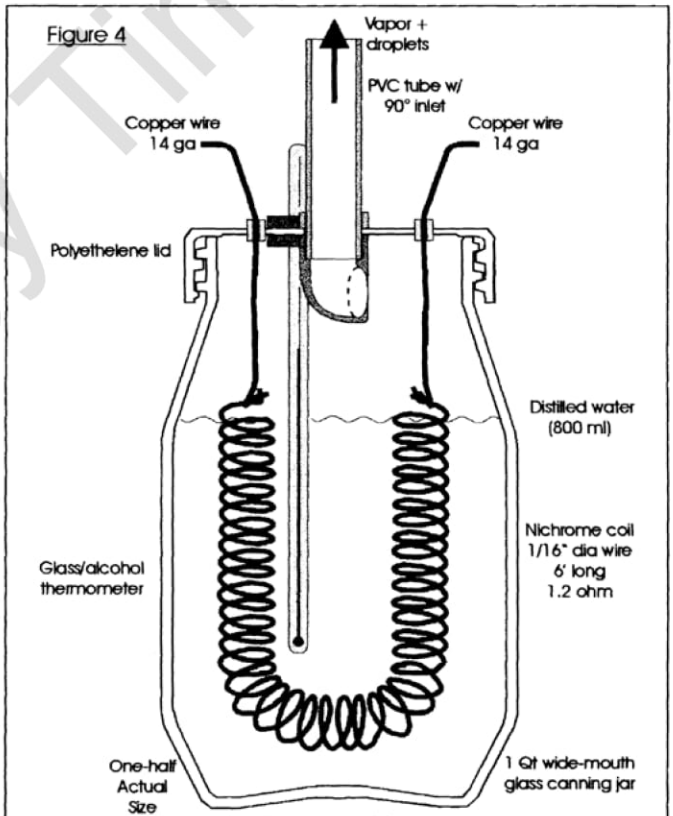


Note: One of the anodes is out of the plane of the drawing; that is why 4 + 2 cm is not a contradiction.

Carbon-electrode Plasma-electrolysis Reactor



Condensing System
Multi-kilowatt capacity



Calibration Heater
One kilowatt maximum

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Stellar Nucleosynthesis

A protostar—an interstellar dust cloud that is gravitationally collapsing and heating under its own weight—begins thermonuclear "burning" of hydrogen into helium when the temperature at its center reaches the ignition point of this fusion reaction. The thermal energy released serves to temporarily hold back the inexorable contraction. Only in the core of the star is it hot enough to maintain fusion reactions. When the core runs out of hydrogen, it again begins to collapse, heating further until helium ignites. The increased temperature of the core during helium burning causes the outer layers of the star to expand enormously and cool to form a red giant. The major ashes of helium burning are carbon and oxygen, and it is widely accepted that the ^{12}C and ^{16}O in galaxies began as solar winds of red giants. In stars below a mass called the Chandrasekhar limit, such as our Sun, this stage is where the nuclear evolutionary process is believed to stop, for once their helium is consumed, mid-sized stars are not massive enough to crush the remaining core into carbon ignition. The nuclear fires go out. All that remains of the former red giant shrinks to a very dense cinder of carbon and oxygen the size of our planet, with most of the star's original mass intact, cooling slowly over billions of years from a white dwarf to a black dwarf.

Stars above 8 to 10 solar masses develop a carbon-oxygen core that is still surrounded by helium- and hydrogen-burning layers, providing mass to further compress the core. At core temperatures of 5×10^8 K and densities of about 3×10^6 g/cm^3, the star begins burning its carbon to neon, sodium and magnesium (^{20}Ne, ^{23}Na, ^{23}Mg). Each successive phase of burning occurs in dramatically decreasing amounts of time and the fusion of larger nuclei require progressively higher temperatures to overcome the increased Coulomb repulsion. For instance, in a star of 25 solar masses, the hydrogen-burning phase lasts about ten million years, the helium-burning red giant phase, a half million years. Carbon is burned out in 600 years, and is followed by a one-year phase of burning neon to oxygen and some magnesium. The core, now mostly converted to oxygen and magnesium, collapses even further, raising the temperature, igniting oxygen to produce mainly silicon and sulfur, plus some argon and calcium and lesser amounts of chlorine, potassium and scandium—a phase lasting six months. Then, after more compression, the silicon ignites and burns for about a day. The extreme temperatures at this stage of compression produce an intense gamma photon flux, causing photodisintegration of some silicon nuclei, releasing nucleons (neutrons, protons) and alpha particles that are subsequently captured by remaining nuclei. Photodisintegration and nuclear capture are competing processes, effectively rearranging nucleons into evermore stable configurations, building elements up to and slightly beyond the iron peak. At iron, the binding energy per nucleon is at its highest, and no further energy can be extracted by fusion, although heavier elements are produced through neutron capture. A growing core of iron at the center of the star is surrounded by concentric burning shells of different phases, regressing outward from silicon- through to hydrogen-burning at the outer layer. Once fuel is exhausted in a 1 to 2 solar mass core—burned to iron ashes at densities of above 4×10^9 g/cm^{-3}—there is no longer sufficient thermal energy available from any source to resist the inevitable gravitational collapse. The final implosion of the core occurs in seconds (free-fall time), to slightly greater than nuclear density (2.7×10^{14} g/cm^{-3}). The core then bounces back hydrodynamically in milliseconds, sending an enormous shockwave into the outer layers of the star, blowing them into space in a supernova explosion. The high temperatures generated by the shock wave triggers nucleosynthesis of a wide range of both stable and radioactive nuclei in the intermediate layers as they are blasted away, for a few tenths of a second, resulting in the mix of elements which constitute galactic dust clouds, as well as the present experiments, etc.

2. Experimental

Equipment and Setup

The first experiments in this series were performed in attempts to replicate the results of Mizuno and Ohmori, described at ICCF-7, Vancouver, April 1998 and published in the proceedings. These reactions, in particular, seem to this author to have little potential for commercial use heat generation (the opposite is believed true about D_2O-Pd), but seem to hold strong possibilities for "proof-of-principle" of excess heat production and nucleosynthesis. They were said to be quick to run, and to be an entertaining circus of dancing, flashing bright sparks, combined with explosive crackling and thumping sounds—virtually guaranteed to hold peoples' attention. And this field needs—and deserves—a great deal of attention.

We have performed nearly two dozen runs of these type cells to date, for periods ranging from minutes to hours. In addition, about a dozen calibration runs with various resistance heaters have been done in this series. Also, numerous demonstrations have been run without energy-balance and mass-balance data collection. All experiments except two used 0.5M K_2CO_3 (potassium carbonate) electrolyte, in distilled water. The two exceptions ran in 1M $NaHCO_3$ (baking soda). Almost all experiments successfully exhibited the cathode plasma-discharge effect, with most cathode materials and sizes reaching an incandescent phase of operation, the cathode thermally glowing with an orange-yellow color temperature.

The first ten runs were conducted in 250ml Pyrex beakers, starting with about 200ml electrolyte. The cathodes were 1/16-inch diameter "pure" tungsten welding rods. Early trials used long rods extending from the cell and insulated down to the starting electrolyte level. No insulating sleeving was found that would not be blown/burned off, so late runs used 2cm long pieces of tungsten, notched at the top end to better hold a crimped-on platinum lead wire, insulated to the cathode. The anode was 20-inches of fine platinum wire spiraled around a glass rod, except Run 10, which used a type 308 stainless steel rod, 1/16-inch diameter. The electrode leads and a glass thermometer entered the cells through holes in makeshift plastic covers. Attempts were made to determine energy balance by measuring electrolyte loss due to boiling relative to electrical input, measured with two low-cost DMMs (digital multimeters), one reading voltage, the other, current. The power supply, Figure 1, is based on a Variac salvaged from an old dental X-ray machine.

The planned calorimetric technique proved largely unworkable due to the violent boiling generated by the reactions causing fluid loss in splashing, spray, and fine mist carried by the steam. On Run 2, placing the beaker in a saucer caught most of the splashed-out droplets. Even allowing that only half the splashes were recovered, this ten-minute run seemed to show slight over-unity operation. This encouraged seven more runs of this size, all of which failed to produce valid data due to a variety of causes, such as melting lead wires and operator error, as well as electrolyte loss.

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The above problems, plus the observation that the incandescence phenomenon (under some conditions of rate of input voltage increase through a critical range) had an onset region that seemed to travel up the rod, encased in a glowing ring of apparent plasma, both gave reason to build a larger, better-contained cell. A larger cell allows more head-space to retain splashes and a longer cathode for better observation, but proper operation requires anode area to be larger than the cathode, and we had no more platinum at hand. Thus in Run 10, a stainless steel anode was tested, and found to perform without problems.

The second-generation cells use 1-quart wide-mouth glass "home-canning" jars with polyethylene screw caps. These cells at first used 9cm long x 1/16-inch diameter metal rod cathodes — tungsten (Runs 12, 13, 14) and 308 SS (Run 15 and 16)— with 30cm long anodes in coils about 2cm in diameter of 1/16-inch stainless steel, and later, nichrome. Later cells used 2cm long x 6mm diameter pieces of carbon rods as cathodes with nichrome anodes. The most recent two cells, AA3 and AA4 use all carbon electrodes (Alfa-Aesar 99.9995% pure, courtesy of Cold Fusion Technology, Inc.) as shown in Figure 2. AA4's cathode holder extends further into the electrolyte than AA3's. AA4 uses 99.9995% Alfa-Aesar K_2CO_3, thanks also to Cold Fusion Technology, Inc.

The steam vent in the lid connects directly to an insulated droplet trap and a water-cooled condenser, shown in Figure 3, both made with plastic plumbing fittings. Condensate volume collected in the bubbler trap, also in Figure 3, is a measure of steam production, and reveals electrolyte carry-over through an increase in pH (alkalinity). If a particular run of a cell produces more condensate than a reference heater (Figure 4) with the same power and energy input, then that is considered a sign of over-unity operation. It is a measurement of relative COP (coefficient of performance) rather than calorimetry and is believed to be quite conservative—it ignores the energy in the hydrogen and "light show" of the cell and has little chance of false positives. However, it has proved challenging to build calibrators powerful enough to keep up with the increasing power levels drawn by the cells. Extrapolating lower power-level calibration to compare to higher power-level operation invites errors from system non-linearities, particularly at low power—below 200 watts in this system. The latest cell peaked at 155 volts and 6.8 amps, slightly over a kilowatt, but 500 watts (25V @ 20A) is the highest the heater shown has been pushed. Calibration after experiment is still valid, and kilowatt resistance-heated runs will be done soon.

Operation

All cells were operated in a similar pattern. The electrolyte is preheated to about 85°C in a water bath, while at the same time, the system is preheated with the calibration heater. With the cell inserted in the system, the power is turned on at about 85 volts DC and rapidly increased to initiate the cathode discharge effect, if it does not immediately appear. Usually, this first stage of ignition occurs by 95 volts. When it begins, the current drops abruptly. The cell is held at this voltage or slightly higher, until the electrolyte is fully boiling—about 96°C in Denver, Colorado. The current continues to fall as the electrolyte heats. At this stage, in potassium carbonate, with both tungsten and stainless steel cathodes, the discharge flashes have a pinkish hue, while with carbon they appear pure white. In $NaHCO_3$, both tungsten and carbon produce bright yellow flashes.

When the cell reaches boiling, the voltage is increased to about 130-135V and on many runs to as high as the supply will allow—155-165V—but it still draws about the same current, around an ampere per cm^2. At around 130 volts, a new phenomenon takes over, mentioned previously, and, with all but some carbon rods, the cathode becomes incandescent—its radiative warmth may be felt with the hand. The bright, brief but multitudinous sparks change from pinkish to bluish-white in the cases of tungsten and stainless, and seem to become even brighter white on carbon, after the passage of the incandescence-triggering "fireball." That there is a high UV content is attested to by "sunburns" on the experimenter and two associates, caused by the first runs with stainless steel cathodes (Runs 15 and 16). After those incidents, appropriate shielding was installed, and it is highly recommended for any attempted replications that neutron, UV and soft X-ray protection be employed.

3. Results

When running either a cell or a calibration heater, after condensate begins to flow from the heat exchanger, its collection is initiated, the time noted and cell voltage and current is recorded by hand at two to three minute intervals. Condensate is usually measured every ten minutes. With many runs, some lasting as long as 90 minutes, there is much data, and it has not yet been compiled and analyzed. Calibration runs at 750 and 1,000 watts are needed. Only spot-checks of the data have been calculated for this preliminary report. These show indications of some over-unity operation (greater energy output than input) in several cases, but they are probably not greater than 50% excess in the highest. It is likely that the cathodes are contributing chemical energy to the system, particularly in the case of stainless steel and carbon, which produce large amounts of very fine material that blackens the electrolyte (but settles out), and, steam-borne, coats the inside of the de-mister, the heat exchanger and the bubbler.

Run 15, the first experiment using a stainless steel cathode, was also the first to generate the black, finely divided substance. It was at first thought to be just carbon from the breakdown of the carbonate electrolyte, until it was noticed that as the material settled out of suspension, a ferrite permanent magnet could be used to pull it up the side of the jar. The electrolyte was filtered, the residue dried and weighed: 0.43g compared to a cathode weight loss of about one-tenth that. Dr. EugeneMallove, editor/publisher of *Infinite Energy*, suggested having the residue and rod analyzed by SEM-EDX (thanks again!). This analysis showed only iron, chromium, nickel, carbon, calcium, potassium, oxygen and silicon. There were no surprises, except that the metal of the rod had been replaced by a strange, brittle, mineral-like carbon-silicon-oxygen-metal substance, with a fine wire of pliable metal running through the core. This suggested the most recent series of carbon-cathode experiments, of which *totally non-metallic cells* AA3 and AA4 are the latest. Each of these carbon-cathode cells has repeatedly produced black fine-grained substance that holds many grains that are ferromagnetically oriented by strong permanent magnetic fields and that look metallic under magnification at 30 and 250 diameter enlargement. The residue from AA3 (0.47g total from a 20 minute run) yielded a flake 3.3mm x 1.7mm that could be dragged across a sheet of paper by a magnet beneath. Samples were sent to Prof. John Dash of Portland State University in Oregon, who has kindly performed preliminary microanalysis (SEM-EDX) on the residue from AA3 and reports that scans of 10μm areas reveal an inhomogeneous mix of iron, nickel, chromium at 1% levels (plus the expected K, C, and O).

Cell AA4, run on December 5, 1998, used reagent-grade electrolyte in de-ionized water, and was murky-black after

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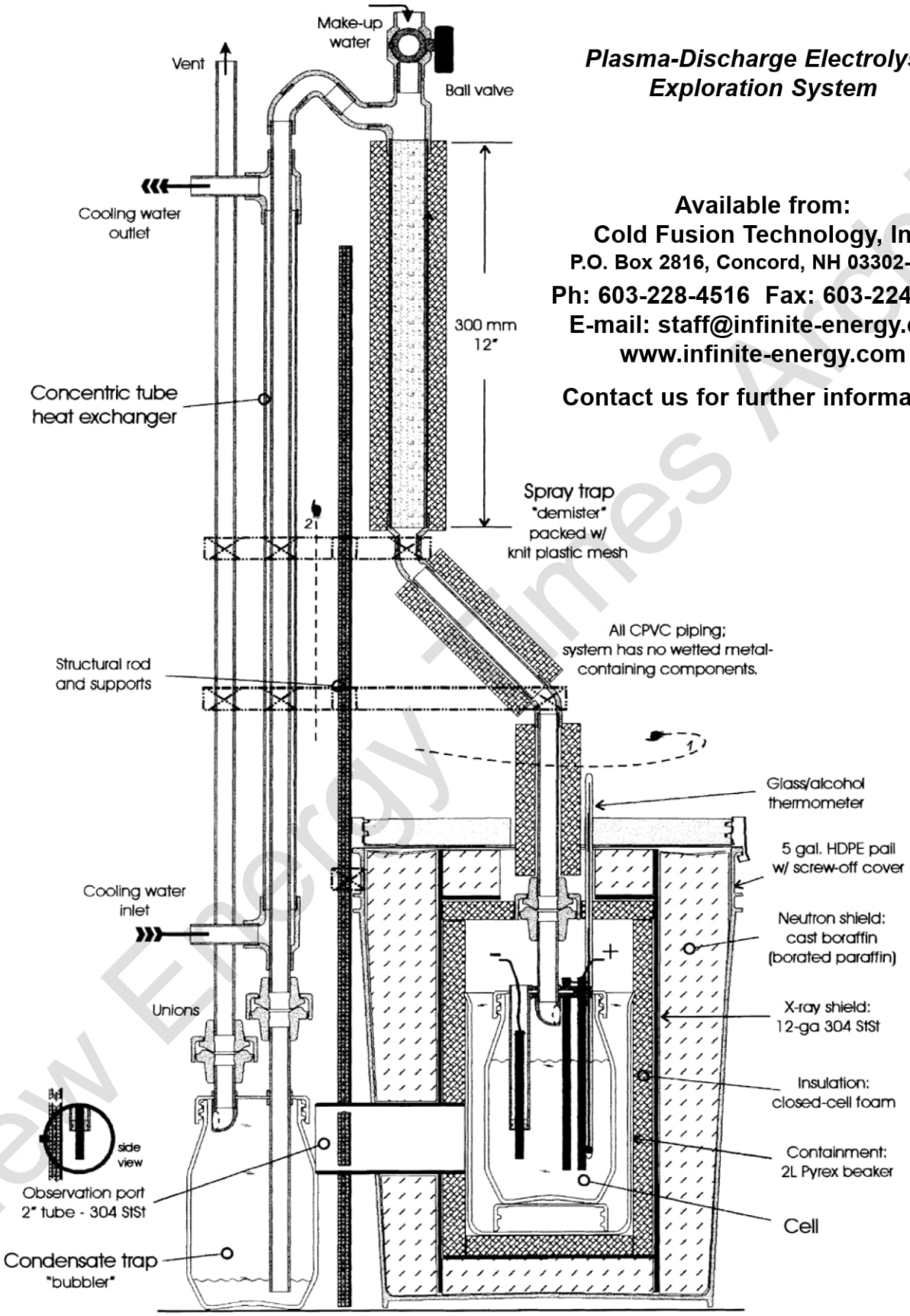

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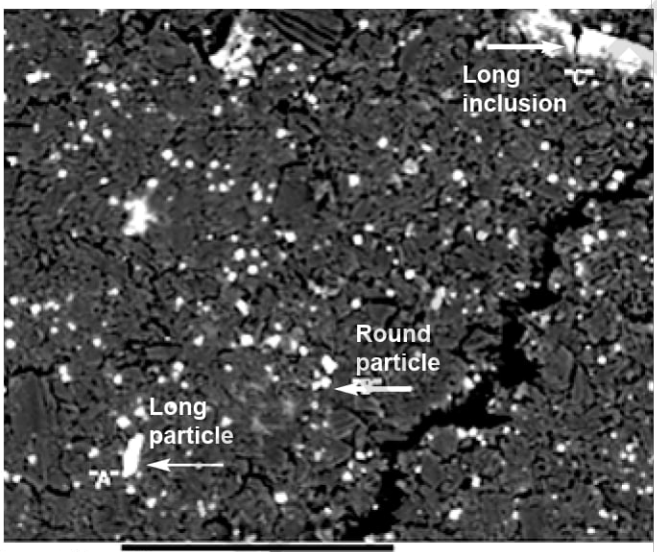
running, but unfortunately the cell jar was thermally shocked in cooling, after the electrode assembly had been removed, and the electrolyte lost. The de-mister, which had been thoroughly backwashed with a garden hose prior to the run, was backwashed with a spray bottle of distilled water. The wash water was filtered and this residue also held many magnetically active particles.

In late December, Prof. Dash analyzed a sample from this recent, "clean" experiment (all materials of known purity). Four out of four randomly selected particles observed gave significant iron peaks (up to 30% iron content), and two of the four particles gave chromium peaks (both about 3%). Dash also photographed the particles. The instrument—a scanning electron microscope combined with an energy dispersive spectrometer—uses electrons to excite characteristic X-rays from solids. (It cannot differentiate between different isotopes of an element.) (Editor's Note: See additional test results below.)

AA4 will be rerun, and residues will be extensively analyzed, including searching for isotopic anomalies.

4. Conclusions

This is an on-going experimental effort, in a fascinating new area of scientific endeavor. No absolute conclusions can be drawn from these experiments at this point. However, it looks very likely that these cells are truly producing macroscopic quantities of ferromagnetic and other elements by some as yet unknown process. Convention dictates that only stars can make iron. These cells are bright in operation, but fall far short of stellar. Much work remains and additional reports will follow as it proceeds.



Sample #1 100 µm

Editor's Note: Further Element Analysis

New Energy Research Lab (NERL) received several vials of material from Chip Ransford taken from the AA4 test. We took these to Geller Microanalytical Laboratory in Topsfield, Massachusetts. Joseph Geller performed an SEM and EDX examination of parts of three of the samples. He used a "JEOL" JAMP-10S machine.

The SEM photo (above) provides a view of the surface of one of the carbonaceous flakes in the residues from the experiment. One cannot draw firm quantitative conclusions from this one-hour's worth of testing. However, an iron peak in the X-ray spectrum was seen on at least one of the particles adhering to one of the flakes of carbonaceous residue. (See spectra for both the top and bottom of this particle.) Also present are aluminum and magnesium lines, as well as prominent calcium peaks.

Results from Professor John Dash

Professor John Dash of the Physics Department at Portland State University in Oregon kindly provided us with the following SEM photographs and EDX element analysis spectra. He made these for several randomly selected, apparently ferromagnetic material-containing particles that were obtained in Chip Ransford's test AA4.

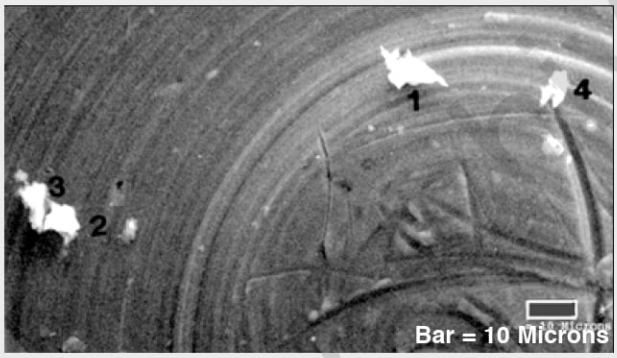
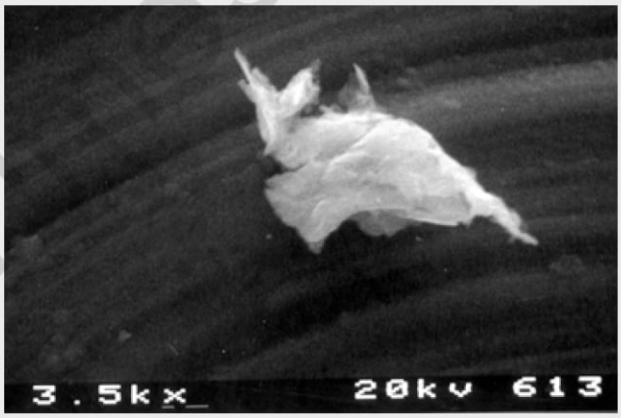
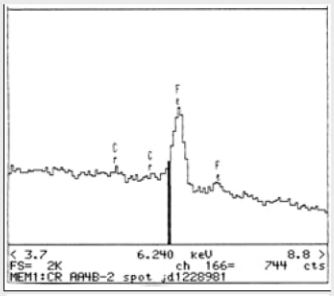


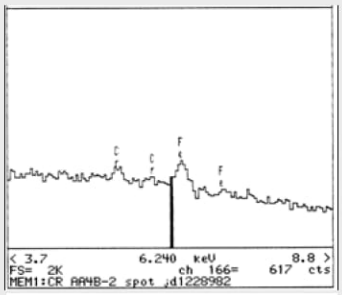
Photo of four particles obtained by Chip Ransford from run AA4. These were analyzed by EDX spectroscopy for unusual element content. *(Courtesy: Prof. John Dash)*



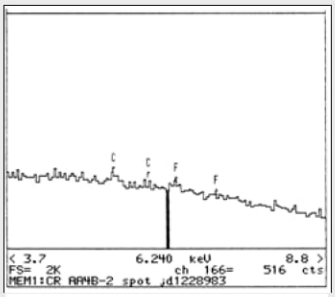
SEM enlargement of particle #1. *(Courtesy: Prof. John Dash)*



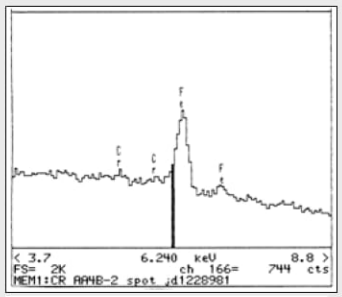
Spectrum from particle 1. It contains about 30% Fe and 1% Cr.



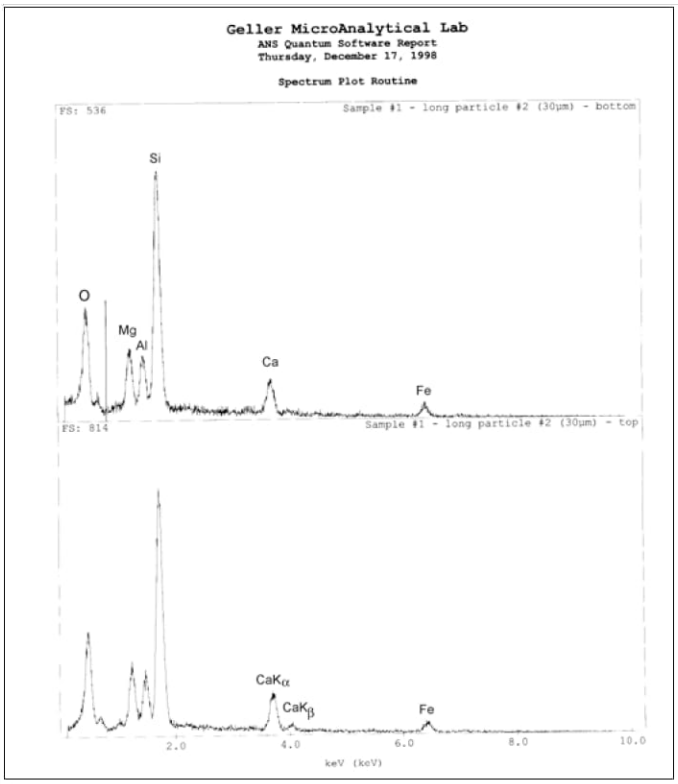
Spectrum from particle 2. It contains about 5% Fe and 3% Cr.

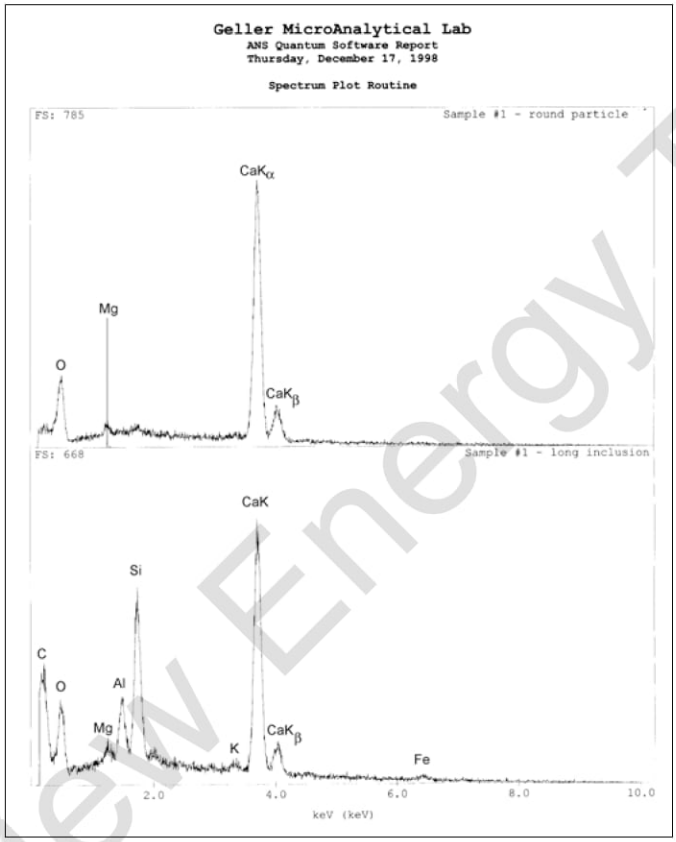


Spectrum from particle 3. It contains about 1% Fe and 1% Cr.



Spectrum from particle 4. It contains about 15% Fe and 3% Cr.





EDX Element Analysis scans for some particles from AA4 material.

spectrum was seen on at least one of the particles adhering to one of the flakes of carbonaceous residue. (See spectra for both the top and bottom of this particle.) Also present are aluminum and magnesium lines, as well as prominent calcium peaks. Examination of a round particle image revealed a larger calcium peak that is significant in its comparative intensity with respect to the adjacent potassium peak. Given that the electrolyte solution from which this residue was derived has so much initial potassium in it, it is surprising to find much more calcium.

We are mindful that some theorists and experimenters in cold fusion (such as Dr. Robert Bush and Robert Eagleton at California Polytech. Institute in Pomona, California and Dr. Reiko Notoya at Hokkaido University in Japan) have claimed to observe the transmutation of potassium to calcium. The latter would require the addition of a proton to the potassium nucleus, or a conversion of a potassium nucleus neutron to a proton.

Clearly there are a large number of questions that further testing and experiments should be able to resolve. Most shocking of all is the very strong evidence of iron production.

"A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it."

"Science only progresses funeral by funeral."

Max Planck, "A Scientific Autobiography (1949) (translated by E. Gaynor)

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DECEMBER 1998 KINETIC FURNACE TEST

Previously Reported Results Retracted

by Jed Rothwell and Ed Wall

We first reported on the Kinetic Furnace, invented by Eugene Perkins and Ralph Pope, in Issue No. 19. The device had, at that time, been tested by several independent engineering laboratories and services. The Kinetic Furnace is, as the name implies, a device for heating and forcing airflow. Heat is generated by means of a rotor that flings water from its hub to the rim of its chamber, through some precisely dimensioned nozzles. This "stirring" action is driven by a 6 HP electric motor. The heated water is driven out of the rotor chamber into a radiator where heat is extracted from it. Another, smaller motor drives a squirrel cage blower, drawing air through the radiator and out an output duct.

In April 1998, Eugene Mallove and Jed Rothwell assessed the furnace for themselves at the inventor's facility in Cumming, Georgia, where they observed the apparent production of excess heat.

Further testing was carried out by Mallove and Ed Wall, June through September of 1998, in Bow, New Hampshire at NERL (New Energy Research Laboratory), but no significant excess heat was observed during that period. Another machine was shipped from Georgia, but it too showed no excess. Finally, Pope loaded a third unit into a van and drove it to New Hampshire himself. He helped install and test it, but this third test also failed. Different sources of water were tested, the operating temperature and the rotation speed of the motor were varied slightly, but no significant excess energy was observed. In *IE* No. 22 we reported on this briefly, expressing continued hope that the machine would produce excess heat. We reported a COP (Coefficient of Performance) of 115% (15% excess heat). This level of excess heat is difficult to establish with certainty using airflow calorimetry. A 200 or 300% excess could be detected with confidence, but 15 to 20% could be the result of subtle errors.

Pope returned to Georgia, discouraged. It was clear that we had hit a dead end, and that if the machine does work, there must be something different about the way it was being operated or the water or some other material in Georgia. We decided that the only way we would ever get to the bottom of this mystery would be to conduct extensive tests on site in Georgia, using our instruments and Pope's in parallel. The machine is large enough to allow several temperature probes and ammeters to be attached simultaneously, unlike the small hand-held cold fusion cells, which often only have room for one set of instruments.

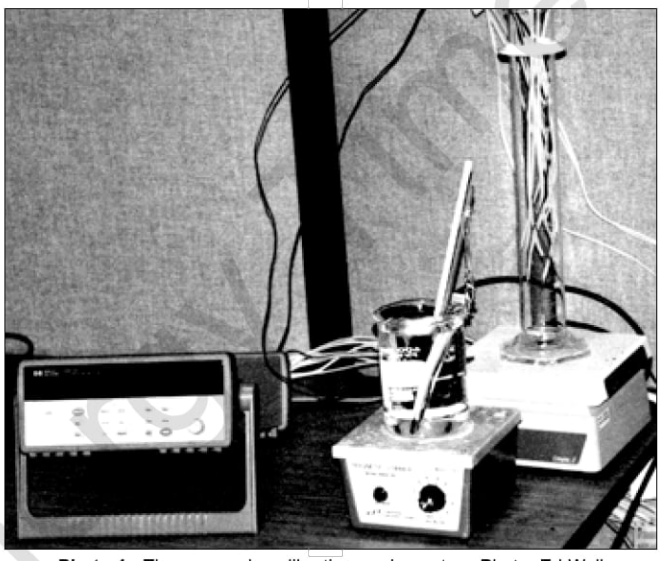

Photo 1. Thermocouple calibration equipment. Photo: Ed Wall

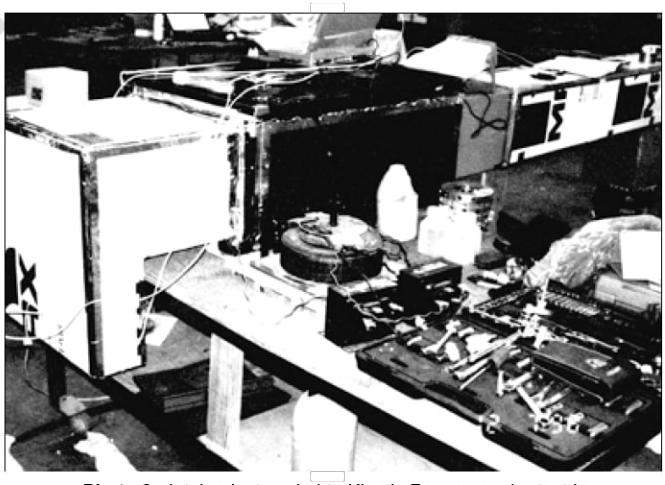

Photo 2. Intake duct sealed to Kinetic Furnace under test in Cumming, Georgia. Photo: Ed Wall

In November 1998, Pope reported that he was now achieving a COP as high as 180% with the machine he had brought to Bow, which had been reconditioned and reassembled with a new rotor and pipes. Rothwell conducted a half-day of testing of this machine in the Cumming, Georgia machine shop location, using the same instruments and techniques Rothwell and Mallove used in April. Most of this 180% turned out to be an artifact of Pope's anemometer, which suffered from a power supply problem caused by worn out rechargeable batteries. The measured air speed was too low. The high excess heat results reported by Pope in previous issues of this magazine were also probably caused by this error. (Ralph Pope does not agree with this assessment and believes that air speed was measured correctly.) The blower power did not change and so it is highly unlikely that the airspeed fell. While the large excess was clearly wrong, apparent 46% excess heat was seen, which is in line with what we observed in April. We were encouraged by this preliminary result, yet puzzled and wary by our inability to replicate it in New Hampshire. We decided to press ahead with full-scale tests in Georgia. Ed Wall went to Georgia bringing several tools and precision instruments, listed on page 27.

In the series of tests from December 4-9, 1998, Wall, Rothwell, and Pope tested the Kinetic Furnace extensively, using higher quality instruments and more sophisticated techniques than Pope had ever used. Unfortunately, no significant excess heat was observed. Based on the December results, we believe our initial assessment in April was incorrect, and there was never any significant excess heat in the tests we performed either in Georgia or in New Hampshire. We believe we have discovered the source of the error which caused the artifactual excess heat in Georgia. The error was in technique

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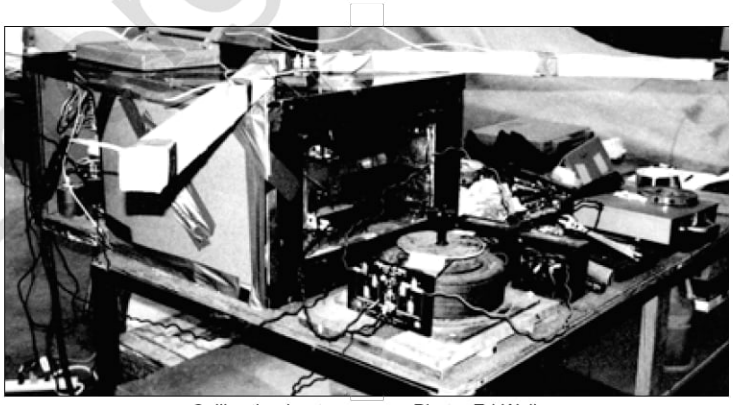
rather than instruments or formula. In the December tests we used an improved technique, a computer, an HP 34970A Data Acquisition System, and an array of 11 K-Type, 20 gauge wire thermocouples (four on the inlet and seven on the outlet side). The thermocouples were calibrated carefully through the temperature range of interest and compared to NIST traceable mercury thermometers. By performing this calibration, we learned that the thermocouples read about 0.5°F less than the calibration thermometer over the temperature range of interest (see Photo 1). At the same time we used the computerized instruments, we repeated the tests using the same relatively crude, hand-held instruments—ammeters and thermometers employed in November. In this second test with hand-held electronic, alcohol and mercury thermometers, we measured no excess heat, thus confirming the computer thermocouple readings.

Pope's Pitot-tube anemometer with our anemometer. Thermocouples in background. Photo: Ed Wall

The biggest problem with the April and November tests in Georgia was the lack of a calibration heater. This was not an error or an oversight—we did not have time to install one during these preliminary, one-day tests. The tests in Bow, New Hampshire, were conducted over two months, and they employed a calibration heater to avoid dependence on air speed measurements and formula-based calculations. With a calibration heater, results from the electric heater are compared with those measured with the Kinetic Furnace. Even if the air speed, electric power, or duct cross-section measurement is inaccurate, the comparative results should show an excess, if one exists.

Jed Rothwell checking output air temperature. Photo: Ed Wall



Calibration heater set-up. Photo: Ed Wall.

The first day and a half of testing in Georgia were devoted to installation and testing of the thermocouples and the calibration heater, which was run at three power levels, up to 3.25 kW. At the end of the second day we turned on the Kinetic Furnace, which also consumes about three kilowatts electric power. All tests were done with the heater in place, whether it was active or not, to maintain consistent air flow patterns. The Kinetic Furnace testing protocol calls for the machine to be run with the cooling fan turned off until the internal water temperature rises to at least 160°F. Then the blower is turned on, and internal temperature drops rapidly at first. Stored up heat in the rotor and water are removed. In 20 to 30 minutes the rotor and outlet air temperatures stabilize. Twenty minutes into the first Kinetic Furnace test, the initial burst of saved up heat was exhausted, and the temperature fell to about the same level seen with the calibration heaters at 3 kW. It was obvious that the furnace was producing no excess heat.

In our first test it was apparent that the Kinetic Furnace was producing no excess heat. This left two possibilities, which we investigated over the next five days:

1. That the previous results were an artifact.
2. That the machine previously produced excess heat, but it was not producing it on December 4.

To check for possibility #1, an artifact, we began by repeating the tests with the thermometers, hand-held ammeters, and other instruments used in the previous tests. We placed the thermometers in the same locations as the computerized thermocouple arrays. The hand-held instruments were used at the same time as the computerized equipment, during both calibration heater runs and live Kinetic Furnace runs. The hand-held instruments showed the same 9 or 10°F ΔT as the computerized thermocouples, which indicates no excess heat. We then moved the thermometers to a location roughly as far away from the Kinetic Furnace as Rothwell selected in November and we observed a 13 or 14°F ΔT. To assess possibility #2, we tried changing the rotor, the water, the air flow speed and other parameters, which we hypothesized might have a controlling effect on an excess heat phenomenon.

A hypothesis discussed by Horace Heffner in the Vortex Internet forum came to mind. Heffner thought that a warm stream of air might be moving from the outlet duct 15-feet back to the inlet. Although that seemed unlikely, we looked for a stream of air by placing the anemometer next to the outlet duct, at a spot 50 cm back from the end of the duct, toward the Kinetic Furnace. We moved the impeller around, searching for a stream of warm air, checking the left side of the duct, the right side, the top and bottom. The anemometer is quite sensitive to small streams of moving air. The impeller did not spin, so we conclude there was no discrete stream of air going from the outlet duct back toward the Kinetic Furnace. However, the

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hypothesis stuck in mind, so we did a more careful examination on the air surrounding the Kinetic Furnace and duct on all sides. We now believe there is an area of circulated air around the machine that is warm in comparison with air in the greater volume of the room. This was more apparent during tests on Sunday when the machine shop was deserted and the air in the rest of the building was quiescent. (The machine shop is a 5,000 square foot steel-frame building with the ceiling 14 feet high at the eaves.) Outside of this envelope of warm air around the machine, at locations 20 and 30 feet away, the ambient air temperature was roughly 13° cooler than the Kinetic Furnace outlet, and roughly 3° cooler than the air surrounding the inlet. Thus, the actual ΔT temperature between inlet and outlet was 9 or 10°, indicating no excess heat.

In April and November, we measured the inlet temperature at a spot too far from the Kinetic Furnace, outside the cloud of warm air. This spot was picked because Ralph Pope cautioned us not to place the sensors too close to the furnace where they would pick up heat radiating from the rotor and other hot machinery. However, this was incorrect. There was little significant radiant heat; most of the heat near the machine was convective, and it went away during the test. In the first round of tests in December, four inlet thermocouples and three thermometers were placed in various locations around the inlet. The closest ones were about six inches away from the rotor and calibration heater. The farthest ones were 35 inches away from the inlet and reasonably well-shielded from radiant heat, yet they were only 0.9°F cooler after the fan was turned on. The difference would have to be 4° F if the excess heat was as high as it appeared to be in November, so radiative effects are not large enough to nullify the apparent excess heat. During the warm up phase of the experiment, before the fan was turned on, the difference between the inlet thermocouples and thermometers was 2 to 3°. Evidently this was convective heat, because when the fan was turned on and the air pulled past the thermocouples and rotor this temperature difference largely disappeared.

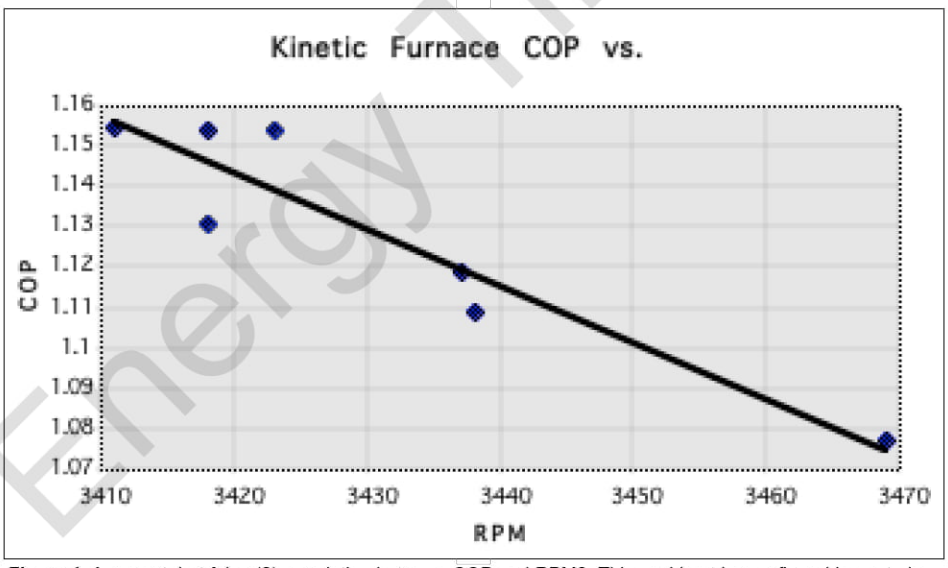

Figure 1. Apparent, but false (?) correlation between COP and RPM? This could not be confirmed by a study of delta-T vs. RPM or delta-T vs. Input Power.

The confusion about the inlet temperature underscored a serious weakness in our test set-up that continued even after the first round of December tests. We were still not doing the calorimetry the way a heating and air conditioning (HVAC) engineer tests a furnace. The HVAC engineer places the inlet temperature sensor at the furnace return duct, where inlet air is concentrated in a single point source. In our tests we did not know precisely where the inlet air originated because we did not have a concentrated point source. When we realized this, we constructed an inlet duct (see Photo 2). The inlet was initially about 20" x 6" located 6" below the bottom of the furnace, in a source of cool air. We believe there is no heat path from the Kinetic Furnace rotor or the calibration heater back to the thermocouples. In runs with the calibration heater, the heat balance computed according to the formula came out close to unity, with a COP between 96 and 106%. This inlet duct draws warm air from the cloud surrounding the Kinetic Furnace and its environs, but that makes no difference.

After installing the inlet duct and making other improvements, we tested intensively for three days. Pope altered the pump several times, changing out the rotor and water, but these changes had no effect, just as they had had no effect in Bow. Based on these tests and the exhaustive testing in Bow, we conclude that the three machines we have tested never produced excess heat. It is possible that a Kinetic Furnace produced excess heat in earlier tests at Pope's facilities with the Air Techniques engineer, or in tests at Dunn Laboratories, Inc. and elsewhere. Pope reports that during these tests, the outlet duct was always passed through a plywood barrier in a window and vented outside, so the error we observed in December could not have occurred.

Rotor heat up rates were similar to those measured in Bow, and rotor steady state temperatures were nowhere near those reported by Pope (140 - 150° F). Such high temperatures would be difficult to explain, except as apparent and strong excess heat, but they could not be confirmed. This steady-state rotor chamber temperature remains a key unresolved issue. If there are conditions during which this temperature is higher than what we have seen, then it is possible that Pope and Perkins saw better results. Attempts were made to increase the rotor temperature by restricting the intake plenum cross-section area. The rotor temperature was raised ~10° F by this method, but this introduced another factor. The air moved much faster in the intake than the exhaust, so it was cooled by the Bernoulli effect. This was seen during calibration when the blower alone was run for an extended period. The COP came out slightly over unity for the blower alone because we did not take into account the Bernoulli equations. The actual COP must obviously be under-unity for the blower.

Why It Took So Long

The reader might wonder why it took nine weeks to confirm the conservation of energy. Our test results in New Hampshire showed no significant excess heat at any time, and the first test of the Kinetic Furnace in Georgia conclusively proved there was no excess heat. On the face of it this is a simple, straightforward measurement very similar to those conducted by heating and air conditioning engineers every day, so you would think that an experienced engineer would get it right the first time with ease. Indeed, Mallove and Wall did get it right the first time. They spent the next nine weeks making sure. The installation in Bow included an inlet duct, so the apparent excess cannot be caused by the same problem we fixed in Cumming. However, it was clear that

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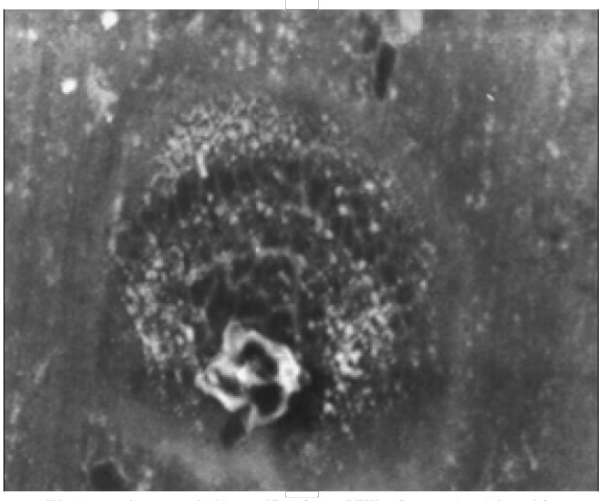
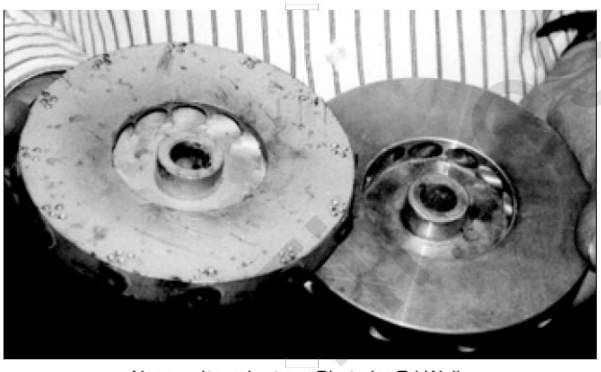
lem we fixed in Cumming. However, it was clear that the calibration heater was also producing noisy, nominally over-unity results, putting 15% within the range of error.

The apparent correlation of rotor RPM to slightly over-unity COP turned out to be unconfirmed by a large number of other tests (see Figure 1).

Another reason it took so long to resolve this issue is that people think slowly and research takes time. Consider the electric generator and motor. Oersted discovered that electric currents produce magnetic effects in 1820. This triggered intense research by Henry, Faraday, Fresnel and other leading scientists. It took Faraday roughly ten years to prove the converse: that magnets induce electric fields. Faraday devised the first crude electric generator in 1831, and it was a while after that before anyone realized that generators can also be used as motors.



Electronmicrograph (magnification x657) of a spot on the side of a rotor that appears to have had a significant thermal event. Direction of water flow is upward here.



New and used rotors. Photo by Ed Wall.

Difficulties

Like most experiments, this was a running battle with recalcitrant equipment, fatigue and inadvertent carelessness. Here are some of the things that went wrong.

At first we measured the power into the resistance heater incorrectly, because of the complicated network of two transformers and the autotransformer (variable voltage transformer).

The power meter interface to the computer failed to work, perhaps because of software conflicts with the HP 34970A, so we were unable to download instantaneous power graphs. We depended on the computed power average and total energy. Power input was very steady so this is not a significant problem. In the previous visit with Mallove, power graphs downloaded successfully and showed steady-state operation.

The computer interface to the anemometer also failed to function correctly. The air speed changed every time we altered the configuration, and twice we deliberately slowed down the blower by re-wiring to increase heat retention in the rotor housing. We thought this might promote excess heat generation. Because we could not record data automatically from the anemometer to the computer, each time we changed the wind speed we had to go through a laborious 20 minute process to manually record the data. It was measured in FPM (feet per minute) with the DTA4000 electronic anemometer. The anemometer was mounted on a camera tripod. The impeller was positioned at nine points on a 3 x 3 array, with points equidistant three inches apart. The impeller was placed at a grid point and left to stabilize for one minute. Eight readings were taken at 15 second intervals. The average value and standard deviation was computed.

After the inlet duct was completed, four thermocouples were installed in various locations within it. Wide variations and fluctuations in temperature were noted. Apparently, eddy currents produced warm spots within the box. All thermocouples were moved to spots exposed to the incoming rush of air, and the temperatures all registered the same. However, they were probably all registering a fraction of a degree cooler than they would have in the same air motionless, because of the Bernoulli effect. This fraction of a degree difference might be mistakenly interpreted as excess heat.

The volume of air flowing through the duct every minute is computed by multiplying the speed of the air, in feet per minute (FPM), by the size of the duct in square feet, to give cubic feet per minute (CFM). With a one square foot duct, one FPM equals one CFM. However the cross-section of this duct was irregular. One side was slightly longer than the others, and the corners were not right angles. We straightened out the corners somewhat with steel angle brackets. We traced the exact inside dimensions of the duct onto a piece of Plexiglas, copied that onto graph paper, and determined surface area, which was 130.7 inches (91% of one square foot). When this correction factor was applied to the formula, the calibration runs and Kinetic Furnace runs agreed to an uncanny extent. The numbers were so close at one point we worried we were making a mistake.

A section of Rothwell's November report describes a typical instrument malfunction:

"At minute 75, I placed the DTA4000 near the stool to measure ambient temperature with the built-in thermometer. At minute 105, I discovered that the milling machine nearby interfered with electronics in the control box. When I lifted the control box, the temperature display changed from 71.6 to 71.1° F. I put it down again and it changed back to 71.6, repeatedly. I moved it a meter away and it dropped to 71.1 and remained stable. The red alcohol thermometer registered 71, and the Acu-rite registered 68.9 and 68.5. I moved the stool two meters further inside the building, to a location where all instruments indicated the air was slightly colder, and all reached the same spread of values they showed before the run: 70.7 on the DTA4000, and 70, 68.7, 68.0 on the others. In the new location, the anemometer moved with a slight draft of 70 FPM. The air was moving towards the Kinetic Furnace."

This illustrates the importance of using instruments based on different physical principles. We use mercury thermometers as well as electronic thermometers because mercury thermometers cannot be affected by the electric fields generated by a milling machine.

How Heat Was Measured

We measured heat from the Kinetic Furnace by two methods. First, we simply compared the control run to the Kinetic Furnace run at the same power level. When the control run temperature went up 9.5°, the Kinetic Furnace went up 9.5°. When

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the flow air was restricted then the control run went up 19°, the Kinetic Furnace also went up 19°. Second we applied the HVAC formula to compute the actual heat flow. The formula is:

> ΔT temperature x 1.08 x FPM (air speed measured in feet per minute by anemometer) x Duct opening as a fraction of one square foot = BTU heat output

Here are two typical Kinetic Furnace runs.

December 5, Run 3

> Input power 3.40 kW = 11,604 BTU/hr. Output power: 10.9 ° F x 1.08 x 1171 FPM x 0.91 sq. ft. = 12,509 BTU/hr COP = 108%

This indicates no excess within the margin of error. In other words, some of the resistance heater control runs are also over 100%, and the standard deviation of the anemometer readings was 46 FPM, so this result was between 106 and 110%. Overall heat recovery from the system was excellent, so you would expect the COP to be in the range of 90 to 100%.

December 5, Run 4

> Input power 3.39 kW = 11,570 BTU/hr Output power: 10.1° F x 1.08 x 1171 FPM x 0.91 sq. ft. = 11,581 BTU/hr COP = 100%

Here is a calibration run with the resistance heater and a different airflow.

December 8, Run 4

> Input power 3.34 kW = 11,399 BTU/hr Output power: 19.0° F x 1.08 x 1022 x 0.55 sq. ft. = 11,534 BTU/hr COP = 101%

Future work, if time and resources allow, will be with water flow calorimetry, which is easier and more precise. Air as a calorimetric fluid is difficult to work with because it is turbulent, compressible, does not mix well, is difficult to meter, it can't be collected and measured easily, and requires a huge duct. The flow of air through the duct varies from one spot to another, and it varies over time. The anemometer impeller is not large enough to cover the entire duct, so it is used to sample the flow at many points. Flowmeters and temperature sensors immersed in a stream of water also test a small sample of the flow at one point. However, a stream of water can be diverted into a graduated cylinder to test flow, and the fluid in the cylinder can be stirred to be sure the probes correctly register the average temperature. You cannot divert the entire stream of air into a container.

Once factors like the size of the duct cross-section were determined with reasonable accuracy, the results from the calibration and Kinetic Furnace runs at different power levels began to line up with unexpected accuracy. For example, in the first set of tests they all showed COP of 96% within 1%. Later, at another air speed, they lined up between 97 and 99%.

Instruments and Equipment

Although the test procedure is simple in principle, we took great care to be sure we were getting the correct answer. One method of doing this is to use redundant instruments based on different physical principles. For example, to measure temperature one can rely upon high precision thermocouples with confidence. In this case we only need to measure temperature to within 2 to 4° F. A cheap thermometer available at Wal-Mart will work adequately for this purpose. We did in fact use some discount store thermometers, and one grade-school science class thermometer. We also used 16 K-type thermocouples, six mercury thermometers (of various different ranges), two bi-metallic dial thermometers, a hand-held, high-precision, high-temperature dual thermocouple (HP-52), and a red alcohol thermometer.

The HP 34970A thermocouple differences, as received, were less than 0.1°. The other instruments did not agree so precisely, varying as much as 3 °F. In one test of ambient temperature, the thermocouples settled at 73.9, 72.3, 72.7, 72.0; the mercury thermometer, which was the most accurate, settled at 72.3; and the red alcohol which is marked in 2-degree increments, indicated 74°. In a test of the outlet duct temperature, the thermocouples and thermometers registered 82.4, 82.9, and the red alcohol thermometer, which had a consistent 2-degree bias at all temperatures, registered 84° F. At that moment the HP 34970A thermocouples registered: 82.6, 82.7, 82.8, 83.0, 83.1, 83.0°. This 0.5° spread of values was real: temperatures within the air stream did vary. The thermocouples agreed more closely when calibrated in stirred water or left in calm, ambient air.

Even though the cheaper thermometers did have pronounced biases, each agreed with itself. That is to say, when we moved a mercury thermometer, a thermistor, and the red alcohol thermometer from the inlet to the outlet, they all rose 9.5°, even though they started at different values. The cheaper thermometers were inaccurate, but precise. ("Inaccurate" means the starting point in the temperature scale—the absolute temperature—was incorrect. Precise means the temperature rose the same extent as the NIST traceable thermometers.)

Equipment used in this test included:

- HP 34970A Data Acquisition system
- 11 K-type, 20-gauge thermocouples
- Toshiba laptop computer interfaced to the HP 34970A.
- A Compaq portable computer to take notes and compute preliminary results with a spreadsheet
- Mercury thermometers to measure ambient air, 68 to 80 degrees F maximum
- Amprobe DM-II recording power meter.
- Pacer Industries, Inc. model DTA4000 impeller anemometer. The built-in thermometer was used in November, and it malfunctioned (See photo of Pitot-tube anemometer).
- Amprobe "Ultra" clamp-on inductive analog ammeter and voltmeter, and a Micronta clamp-on inductive analog ammeter and voltmeter. These instruments do not detect the power factor so they tend to overestimate electric power. However, in the second set of tests in December, the results they showed were close to the power measured with the more sophisticated Amprobe DM-II.
- Acu-rite digital electronic thermometer with two thermocouples.
- Bimetallic dial thermocouple to measure ambient temperature.
- Red alcohol thermometer from ABC School Supply, Inc.
- Dial thermometer on rotor chamber to measure the water temperature.
- Two ducts made from 6' x 4' sheets of building insulating material.
- Stopwatch.
- Electronic camera, which is invaluable for this kind of test.
- To calibrate, a variable voltage autotransformer, two transformers, and a duct heater with 3.2 KW maximum output.

Was It Worth It?

We wrote above, "the actual COP must obviously be under-unity for the blower." A cynic might say that the actual COP for a water mixer must also obviously be under-unity, our tests

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were in vain, and we made a gargantuan effort to prove the conservation of energy and the fixed ratio of work and heat. This ratio was established in the 1840s by J.P. Joule. He used a falling weight to drive a paddle that stirred water and raise the water temperature. It sounds similar to the Kinetic Furnace—it sounds as if we are trying to overturn an observation established a hundred and fifty years ago and confirmed countless times every day by scientists and HVAC engineers everywhere. But, there is an important difference between Joule's experiments, stirred water, and ours. The stirrer in the Kinetic Furnace rotates much more quickly than Joule's, so quickly that it almost certainly creates cavitation. Similar cavitation devices on the smaller scale have apparently produced excess heat and nuclear effects. The nuclear claim is controversial, but widely accepted. Much of the investigation into apparent nuclear effects caused by cavitation is being performed in the mainstream by conventional scientists, and approved of by the *New York Times*, *Scientific American*, and *Popular Science* (*e.g.* "Star in a Jar: Can a Bizarre Transformation of Sound into Light Trigger Fusion in a Small Flask of Water?" by Arthur Fisher, *Popular Science*, December 1999). The Kinetic Furnace and the Griggs HydroSonic Pump probably perform cavitation on a scale thousands of times larger than any of the experimental sonoluminescence devices. We must say "probably," because we have no direct proof that cavitation is occurring, because we cannot see inside the steel chamber. Perhaps the Kinetic Furnace was previously cavitating and producing excess heat, but it later stopped.

It would be absurd to question the validity of J.P. Joule's experiments. Cavitation has been carefully studied since it began damaging marine propellers about 150 years ago. But, as far as we know, cavitation and heat together have not been carefully researched. People have felt no need to study heat evolving from cavitation, because no one suspected the heat might be unusual. Science works a little like a national park. Thousands of people cluster around the main attraction and the visitors' center. Hundreds hike down the nearby well-worn paths, measuring heat and cavitation. But, the moment you step off the path into the woods, you leave the crowds behind. In a national park it is unlikely that you will stumble onto an unexplored rock formation or a hill that has never been climbed, but in a quiet spot you might find a fossil or new species of insect. The unexplored avenues of science are infinitely larger than the physical paths on earth. The lesson of cold fusion, the Marinov motor, and other strange phenomena described in this magazine is that you can reach the unexplored wilderness of science in a few minutes with simple tools.

Previous Tests and Recent Work by Pope and Perkins

The Kinetic Furnace reportedly produced large excess heat in other tests over the years at Dunn Laboratories, Inc. (1982, 1983), Pittsburgh Testing Laboratory (1984, 1986), Automated Test Labs (1986), and elsewhere. What happened during these tests? Were the professional laboratories incorrect? We do not know. In the papers provided to us by Pope, the tests are not described in enough detail to judge with finality. It seems unlikely that professionals in these laboratories made the same kinds of mistakes we did initially, before we installed the inlet duct. After all, their business is to determine the COP of furnaces. However, they never pursued development of the Kinetic Furnace. That is inexplicable behavior. Other companies in the United States that tested over-unity cold fusion devices have been quite enthusiastic. Heating and air conditioning companies have often contacted our magazine and asked whether any practical device is available. They seem anxious to proceed with development, and totally unconcerned about the fact that the scientific establishment does not believe these devices exist. This is speculation, but perhaps after Dunn Laboratories and the others wrote the reports provided to us by Pope, they realized that they might have made a mistake of some sort. The HVAC engineer in Atlanta who performed the tests on the Kinetic Furnace many years ago stood by his work, but he explained that it was a preliminary test.

It is possible that for the past few years, Pope and the late Eugene Perkins were performing invalid tests, and their results may have been meaningless. They never employed a resistance heater or an inlet duct, so they never would have caught the errors we discovered. They did not keep adequate records by our standards, they had no computerized data collection, and they did not organize their tests in a methodical, step-by-step fashion. To their credit, they did the best they could at the time under difficult circumstances.

Their open and cooperative attitude, and their willingness to honestly face the facts is extremely laudable. Many inventors of exotic technology will refuse to allow their machines to be tested in the first place, and even when you find an error with a machine, most will refuse to listen or believe it. Ralph Pope debated the issue with us at first, and he demanded rigorous proof that the inlet temperature was not affected by radiant heat. This forced us to devise a good test to prove our point, with the inlet turned 90-degrees downward and the thermocouples shielded from the furnace above. Pope accepts our conclusion that the present set of experiments show no excess heat, but he believes previous experiments were successful. He intends to continue testing if he can, and we will do so too as time allows.

These results must be seen in light of the James Griggs HydroSonic Pump excess heat claims in a superficially similar device. Mallove and Rothwell made measurements on the Griggs machine in early 1994 (See summary below). The Griggs results may support the idea that cavitation excess energy is real but highly variable, for reasons still not understood.

HydroSonic Pumps have not yet been replicated widely. However, Griggs used much better instruments and techniques than Pope-Perkins, and he uses water flow calorimetry, which is easier and more reliable. We have a HydroSonic Pump, and we intend to press ahead with our plans to test it at NERL when we have the time and resources in 1999.

Should anyone be devoting weeks and thousands of dollars to testing this sort of claim? Mainstream science says no. We think it is worth doing. We are disappointed, and we have no immediate plans to continue testing the Kinetic Furnace at this time, but we do not consider these last few months a waste of time. The instruments have been quite useful with other projects and the skills and techniques for air flow calorimetry may prove valuable.

Testing of the HydroSonic Pump Rotary Cavitation Device by Eugene Mallove and Jed Rothwell (1994)

RESULTS

TEST 1 January 6, 1994 11:30 a.m.

When we arrived, James Griggs explained that he was having trouble boosting the flow rate and maintaining pressure on the unit, so he was not getting a measurable effect. However, he had managed to balance input and output, and to bring the machine into a steady state, so we decided to let it run for an hour producing little or no excess heat, as a "blank" or null test run of the calorimetry. The flow rate was below the window, at

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0.05 gallons per minute.

Results were as follows:
Starting mass and temperature of water in steel drum: 350 lbs, 55°F. Water in input hopper also 55°F.
Ending mass and temperature in steel drum: 376 lbs, 127°F
Water temperature Delta T: 72°F
Energy added to water: 72°F x 376 lbs = 27,072 BTUs, which equals 7.93 kWh

It is important to remember that all of the water that ended up in the steel drum was tap water starting at 55°F. Ambient temperature was slightly higher, at 63°F, but this large mass of water could not absorb any significant amount of heat from ambient in spite of the 8°F difference, because it was heated above ambient by the pump 6 minutes into the test.

Dranetz input power: 13.46 kWh Dranetz PF: 73%

COP computations
(COP is output energy divided by input expressed as a percentage)

Input KWH	COP Apparent
13.46	59% Adjusted for PF
9.83	81% Adjusted for PF and Motor efficiency
8.12	98%

Conclusion: This is close to a balance of input and output. Because there must have been significant radiant losses, with no excess heat, I expect the COP would be lower than 98%, so these results might indicate a small effect.

TEST 2 January 6, 1994 3:00 p.m.

In the afternoon, after the machine turned on and warmed up for five or ten minutes, Griggs and the others tinkered with the input and output flow valves and some other parameters, and after about 20 minutes in all, they announced that the flow was steady at 0.20 gallons per minute, and the power draw kilowatts had fallen, so the effect was turned on. The valve venting the steam outside was shut, the valve leading into the steel drum was opened, and we collected the steam for 19 minutes, 40 seconds. The run was terminated when a circuit breaker in another part of the building shut down the controls. The main power feed did not fail, but it is held on by solenoid actuators, which opened up. The recording Dranetz meter has a battery back up, so no data was lost. All other data collection is by stopwatch and pen on paper. (Events like this remind us that sometimes, the old, simple ways of doing science are the best ways.)

Results were as follows:
Starting mass and temperature of water in steel drum: 350 lbs, 53°F.
Ending mass and temperature in steel drum: 381 lbs, 103°F
Water temperature Delta T: 50°F
Energy added to water: 50°F x 381 lbs = 19,050 BTUs, which equals 5.58 kWh
Dranetz input power: 4.80 kWh Dranetz PF: 84%

COP computations

Input KWH	COP Apparent
4.80	117% Adjusted for PF
4.03	138% Adjusted for PF and Motor Efficiency
3.33	168%

Conclusion: Excess heat was detected at levels far beyond any reasonable error limits for the instrumentation used. If the equipment had been performing the same as it did in Test 1, the final water temperature would have been closer to 80°F than 103°F. This is computed as follows: 4.80 KWH Apparent is delivered to motor, adjusted for PF and efficiency, would have created 3.33 KWH of heat, which equals 11,372 BTUs, which would have raised the 381 lb mass of water by 30°F, but it went up 50°F, instead. I am certain that even my kitchen cooking thermometer can detect the difference between 80°F and 103°F.

This test ran for one-third the time of Test 1. The flow rate was 0.20 gallons per minute, compared to 0.05 g.p.m. in Test 1. The improved PF was because the motor was carrying a greater load with the greater flow rate.

TEST 3 4:04 p.m.

Test 3 ran normally for 30 minutes.

Results were as follows:
Starting mass and temperature of water in steel drum: 350 lbs, 53°F.
Ending mass and temperature in steel drum: 392 lbs, 122°F
Water temperature Delta T: 69°F
Energy added to water: 69°F x 392 lbs = 27048 BTUs, which equals 7.92 KWH
Dranetz input power: 7.26 KWH Dranetz PF: 84%

COP computations

Input KWH	COP Apparent
7.26	109% Adjusted for PF
6.10	130% Adjusted for PF and Motor efficiency
5.03	157%

Conclusion: Nearly as much excess heat as Test 2. Again, the results are far above any possible experimental error.

Addendum to Kinetic Furnace Report by Ed Wall

Examining our lab book after returning from Georgia, I found that the following specific values of COP were measured with the improved inlet duct configuration:

On 12/7/98, Run 1 shows a COP of 1.155. The temperature difference of 12.69 ° F was obtained from 4 input temperatures that have a sigma of 0.51° F and 7 output temperature that have a sigma of 0.16° F. This was the first run done with the new input duct in place. The side of the Kinetic Furnace was closed. CFM was calculated by first re-measuring the airspeed as usual, then multiplying by the factor of 0.9 because of the less than 1.0 square foot cross-sectional area of the duct. Sigma for airspeed was 29 FPM, or 3%. Calibration should have been run right then, but was not.

On 12/7/98, Run 3 shows a COP of 1.19. The blower wiring was changed to medium speed in order to increase the rotor temperature. At the same time, a "ramp" was placed in the output duct to reduce the cross-sectional area. The new CFM was measured to be 781 with a sigma of 11.28 or 1.4%. The delta-T of 15.121° F was from an average input of 71.922 (sigma = 0.095) and average output of 87.044° F (sigma = 0.095). Calibration should have been run right then, but was not.

On 12/8/98, the input air cross-section was reduced. At first, this resulted in a problem with a large difference in readings between the thermocouples because some of them were in eddys and some were in mainstream. This was fixed so that all were reading very close to the same temperature. Run 1 for this data has a COP of 1.148. The input was 70.696°F (sigma = 0.2336) and output was 86.666 (sigma = 0.1773) for a delta-T of 15.970 F. The CFM was checked again and found to be 701.7 (sigma = 11.7 or 1.7%).

Run 2 for 12/8/98 had the intake further reduced and retake

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Water Stirring Discovery: Is It Connected with Cavitation Excess Energy?

by Eugene Mallove

The mysterious properties of water continue to surprise us. From Professor Kazunari Domen's chemistry group at the Tokyo Institute of Technology comes a new bombshell. It just may have something to do with the comings and goings of anomalies in thermal power production from cavitation-type devices—such as the Schaeffer device and the Griggs HydroSonic Pump (see *IE* No. 2) and more recently the Kinetic Furnace (*IE* No. 19 and following issues, including the present), all of which have over-unity performance that continues to be questioned.

As reported in *New Scientist* (November 14, 1998) Kazunari has found a way to split water molecules at room temperature into hydrogen and oxygen, using a simple catalyst—*powdered* cuprous oxide—and stirring the water. The Japanese investigators say that the energy to break water's molecular bonds seems to come from stirring, but I believe that at this point it is really not possible to know exactly from where the energy is coming. Domen says that less than 5% of the energy of stirring is involved in splitting the water. He says that increasing the efficiency of the process ten-fold might lead to practical applications. Perhaps he means a more efficient method of hydrogen production than by energy-intensive electrolysis.

A further note of significance about the work: The Nikon Corporation is reported to be working on the process in parallel with Domen.

Earlier in 1998, Domen had claimed that powdered cuprous oxide catalyst could split water at room temperature if it were activated by visible light. However, now his group has determined that the reaction continues completely in the dark while the water is being stirred—for up to hundreds of hours! Moreover, the more energetic the stirring, the more hydrogen and oxygen are produced, according to the claim. This work is described in the scientific journal *Chemical Communications*, p. 2185 (1998) The earlier light-activated report by Domen *et al.* was in the same journal, Vol. 70, p. 287.

The allegation from Domen is that mechanical energy in stirring is converted to chemical energy that effects bond-breaking. Domen was quoted in *New Scientist* (November 14, 1998), "This is the first example of mechanical energy being converted into chemical energy by catalysis."

New Scientist editorialized: "To most chemists, the idea is heresy. And the prospect of producing a source of fuel by a mechanism no one understands evokes memories of the 'cold fusion' saga of 1989." Ah, but the magazine is kind to him in suggesting that his results were "carefully reviewed before they were published" and in praising him for not "making any bold claims about practical applications." Heaven forfend that any scientific discovery that threatens orthodoxy should at the same time imply practical applications!

Though the announcement has met with general disbelief among chemists, others offer explanations such as: friction between the glass container and water is causing the build-up of static charge, resulting in spark breakdown of the water.

New Scientist quotes Arthur Nozik, a chemist at the National Renewable Energy Laboratory in Golden, Colorado, "I wouldn't be surprised if they are actually using up some of the metal oxide." Yet, the discoverers state that the reaction *is* catalytic and the cuprous oxide is not being used up.

Domen is not suggesting that water contains any mysterious energy source beyond what we might call "ordinary chemistry." Yet it remains an open question what is really producing the bond-breaking. IF the energy required to break the oxygen-hydrogen bonding in water is less than input energy, perhaps we have an over-unity process here.

More to the point, could it be that catalytic particles or compounds within water in cavitation-type reactors are key to producing consistent excess energy? If oxygen and hydrogen are produced by this anomalous low-energy catalysis, and then they reform by chemical combination to water, this would be a possible general mechanism of excess thermal energy effects in water cavitation devices. We'd still be left with explaining how a catalyst can effect an anomalously low-energy bond-breaking, but we might be closer at least to empirical ways to stabilize or enhance the excess energy production of cavitation reactors.

Think about this: Activated carbon with palladium dopant seem inexorably heading toward being completely validated as catalytic facilitators of nuclear fusion of deuterium nuclei to helium, with a corresponding release of thermal energy, *not* gamma rays as expected in conventional physics. Can we really say we understand chemical catalysis at all? Who can say at this point that the cuprous oxide catalyst of Domen might not facilitate an over-unity, non-nuclear reaction such as I have described above? We just do not know enough about the process to say yes or no.

It is very interesting, indeed, that the Domen process does not work with cuprous oxide dissolved in water—only when there are solid particles of cuprous oxide in water suspension. A typical suspension mixture that works—according to *New Scientist* (February 28, 1998) is 0.5 grams cuprous oxide powder to 200 cubic centimeters of distilled water. This was employed in the earlier experiments that were alleged to be activated by visible light (460 to 650 nm wavelength). It has been rigorously determined using ^{18}O isotope-enhanced water that the oxygen release in the experiments is from the water, not the cuprous oxide—hence the allegation of catalytic activity. Catalytic activity persisted for 1900 hours in some tests.

Workers in Japan are not the only validators of the Domen process. In Spain, Antonio Cervilla and Elisa Llopis at the Institute of Materials Science in Valencia have run the process with a molybdenum catalyst. A patent has been applied for on the still-undisclosed catalyst. *New Scientist* (February 28, 1998) quotes Cervilla's remarkable conclusion: "You do not need an energy source to carry out this reaction." And, he says that this hydrogen extraction process is "a revolution in the way of obtaining hydrogen and generating energy in a clean way."

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Joseph Newman's Energy Machine Revisited: Studies by J.L. Naudin and S. Hartmann

by Mike Carrell

Over the last decades, an energy machine developed by Joseph Newman has been the subject of continuing controversy despite repeated demonstrations and a book describing the machine and Newman's theories of its operation. The essential features of the Newman machines are: 1) a very large coil wound with fine wire, resulting in a high inductance; 2) excitation of the coil by high voltage at low current; 3) a rotor consisting of large bar magnets; and 4) a commutator/switch, coupled to the rotor shaft, which connects and disconnects the coil as the rotor turns. Newman's demonstrations typically use batteries consisting of stacks of small cells, totaling several hundred volts. When driving a Newman machine, these batteries show a working life significantly longer than when driving a conventional motor, and, indeed, work when nearly exhausted.

Newman attributes these properties to energy generated within his machine, which recharges the batteries periodically during operation, thus extending their life. Because the performance of batteries is a complex function of the charge/discharge history, these reports and demonstrations have been met with skepticism. (Editor's Note: See also Mike Carrell's treatment of the Newman motor in *IE* No. 7, pp. 54-61.)

Two researchers have studied the Newman machine over the years: Stefan Hartmann in Germany—<http://www.harti.com>,and, more recently, Jean-Louis Naudin in France (<http://members.aol.com/jnaudin509/index. htm>).

Naudin's work is of particular interest, for he has built a small-scale Newman machine, and using a Tektronix digital oscilloscope, plus clever circuitry, has confirmed major aspects of Newman's claims. In particular, Naudin shows: 1) that the machine can force current flow back into the power supply; 2) mechanical power out can exceed electrical power in; and 3) the machine can self-run with no input power for a significant interval (five minutes). All this can be found on his web site.

The purpose of this article is to put Naudin's work into context, for his web pages are notes on work in progress for people familiar with the general features of the Newman machine, rather than a tutorial for a general audience. The machine, while simple in construction, is quite subtle and complex in its operation.

Figure 1 (Ref: http://ourworld.compuserve.com/homepages/jlnaudin/html/qm11bp.htm, QMM mk11) shows the machine, about 20 cm high, 20 cm wide overall, and 10 cm deep. The large coil is wound with 20,440 meters of AWG 34 wire, resulting in an inductance of 220 H and resistance of 17,332 ohms, and weighs 3.65 Kg. Within the coil is a disc, mounted on a shaft with a flywheel to the rear and the switching disc in front. Two stacks of four NIB magnets span a diameter of the disc. The magnetic strength of the magnets is 27 M Gauss-Oersted.

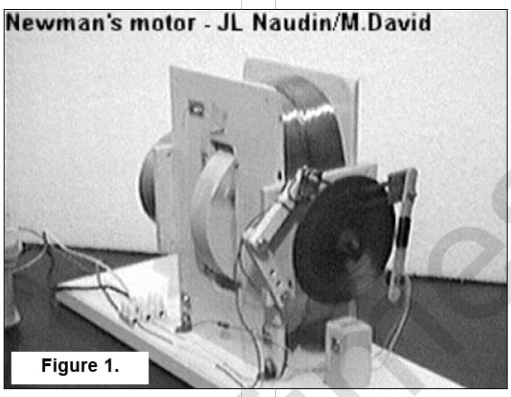

Figure 1.

The switching disc is just an insulating material, on which conductive segments can be glued. A pair of brush fingers are mounted on a hinged arm so that their point of contact with the conductive segments can be adjusted for timing purposes. Multiple segments may be used as shown in Figure 2 (Ref: <http://ourworld.compuserve.com/homepages/jlnaudin/html/Nmac 0625.htm>). As the disc rotates, the coil is connected and disconnected from the power supply by the passing conductive segments. Adjustment of the span and position of the conductive segment(s), and the brushes, gives control over the timing of the system, which is critical.

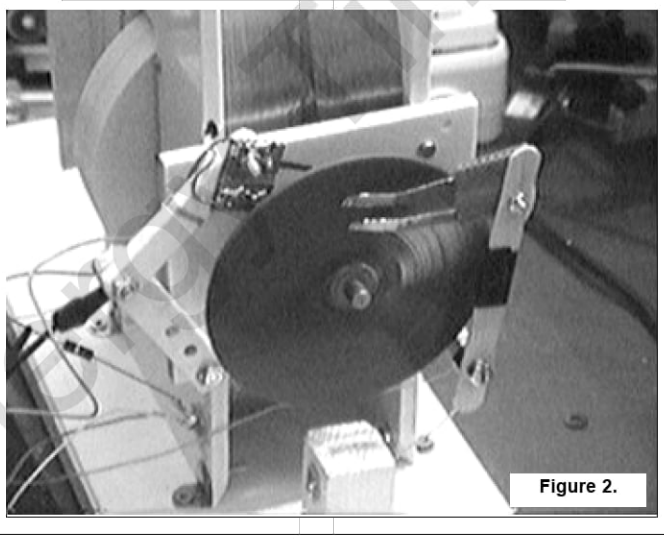
Figure 2.

For research, Naudin substitutes a high voltage power supply for Newman's stacks of batteries. A circuit diagram is shown in Figure 3 (Ref: <http://www.fortunecity.com/tattooine/delany/256/html/nwjlself.htm>, HV power supply & spikes viewer). Note a disconnect point at the secondary of the power transformer, and the storage capacitors at the output of the rectifiers. Note also the circuit for separating current flows toward and away from the coil, with neon indicators showing current flow in each direction. Not shown are a small

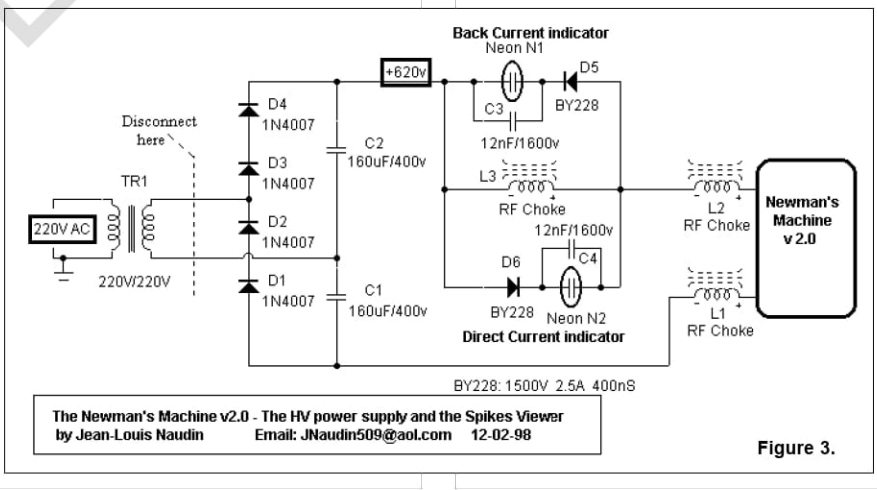

Figure 3.

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capacitor across the coil and resistor shunts used for measuring current flow to and from the coil.

Tektronix THS720P Oscilloscope

This is uniquely suited for measurements of Newman's machine. It is a portable, digital oscilloscope intended for measurements of power in various circuits and AC machines. It runs on batteries, and has an LCD screen. There are two input channels, completely isolated and floating, so they can be connected anywhere without concern about spurious connections. Voltages on each channel are sampled at 500 MHz in exact synchronism, for a 100 MHz effective bandwidth. In addition there is a separate, also isolated, digital voltmeter function. If one channel measures voltage and the other current (voltage drop across a shunt resistor), and the waveforms stored, the instrument will compute instantaneous true RMS power and display it graphically. Stored waveforms can be downloaded to a computer, where further calculations can be performed by complementary software.

This instrument for the first time gives a capability to accurately measure the power content of some of the high frequency waveforms which are associated with the anomalous performance of Newman's machine.

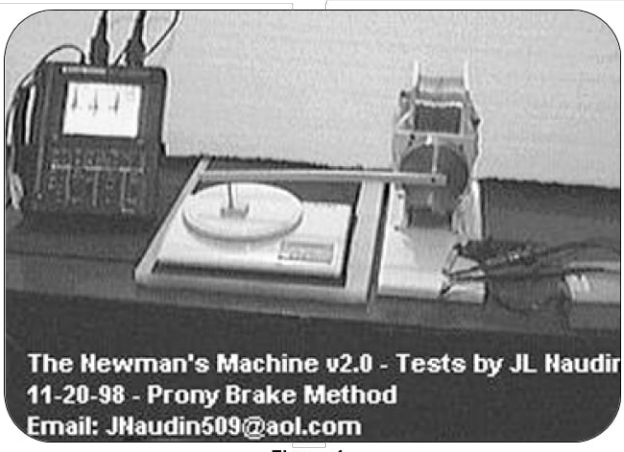

Figure 4.

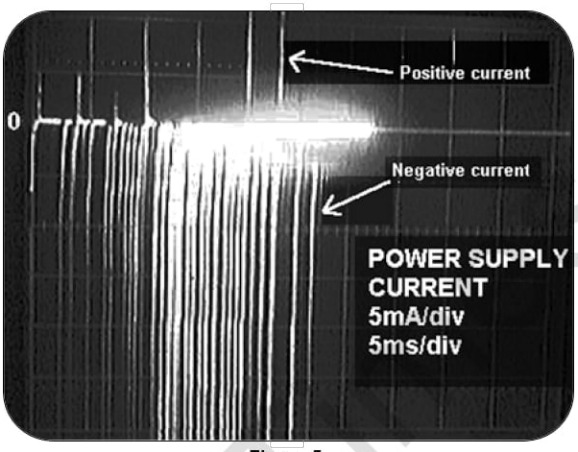

Figure 5.

Newman's Machine Operating Modes

The machine is simple in construction, but its operation involves complex interactions among the following elements. These are not well understood. An extended discussion is beyond the scope of this article. Naudin and Hartmann continue to study them and are posting notes on their respective web sites.

1. Inductance of the coil
2. Current-switching by the contact segments
3. Inertia of the flywheel and voltages induced in the coil by the rotating magnet
4. Arcs drawn between the brushes and contact segments
5. Parasitic capacitance within the coil
6. Character of the magnetic field produced by the magnets
7. Character of the magnetic field produced by the current in the coil

The rotation of the magnet can induce a voltage in the coil equal to the power supply voltage, driving the coil input current to zero. This is not unexpected. If the brushes disconnect the coil before or after this "zero" point, strong sparks are drawn between the brushes and the contact segments. Very complex phenomena occur, in which strong current spikes go from the coil back to the power supply, indicating that the machine has become the source of a burst of power from within itself.

Newman has observed this and demonstrated this for years. Naudin's experiments with a Prony brake and the Tektronix THS720P has given the first quantitative measurement of O/U power from a Newman's machine.

The Prony Brake Experiment

Figure 4 (Ref. http://ourworld.compuserve.com/homepages/jlnaudin/html/nwmechts.htm "The Prony Brake Setup" on p. 2) shows Newman's machine with a Prony brake arm attached, acting on a precision scale, with the Tektronix scope in the background. The oscilloscope and software showed an average input power of 317.1 mW. The mechanical output power measured by the Prony brake was 424.1 mW, an efficiency of 134%. More details of the experiment are shown on the Web page.

The Cooling Effect

With the multiple segments shown in Figure 2, and the brushes adjusted so there is a grazing contact with several segments, remarkable effects begin to appear. One of these is that the coil temperature drops noticeably. This is not quantified by Naudin, but notes from Tim Vaughan on the web site mention that Leon Dragone and Dr. T. Pappas have noted this effect in experiments involving spark gaps and large inductances (not Newman machines).

Oscillograms show strong bursts of current flowing out of the coil in these experiments, as shown in Figure 5 (Ref. http://ourworld.compuserve.com/homepages/jlnaudin/html/NMac1202.htm). The small positive spikes indicate current flowing from the power supply to drive the machine, as one would expect. The negative spikes indicate current flowing out of the machine back to the power supply. These bursts are unidirectional, high frequency pulses. A first consideration would attribute them to the arc drawn between the grazing contact fingers and the multiple segments, but a closer examination leads to more complex, poorly understood phenomena. The cooling effect, the overunity measurements in the prony brake experiment, and the approach to self-running, are all associated with the appearance of these negative current spikes.

Approaching Self-Sustaining

In a series of experiments in November-December of 1998, Naudin disconnected the Newman machine from its transformer (dotted line in Figure 3). Now the only energy available is in the two capacitors. Yet the machine ran for five minutes before stopping as shown in (Ref.: same as for Figure 3, graph on p. 3). Measurements of the power necessary to run the machine give 220 mW. The energy in the capacitors is 15.3 Joules, giving a calculated running time of 70 seconds by conventional reasoning. Yet the machine ran for five minutes.

Summary

Naudin's experiments, reported on his Web page, confirm many of the phenomena claimed by Joseph Newman for his energy machine. It is tantalizing that the Prony brake experiments showed clear evidence of over-unity performance, yet a full self-sustained run has not been achieved as of this date. There is much to be discovered and future issues of *Infinite Energy* will report on developments as they occur. In the meantime, readers are encouraged to study both the Naudin and Hartmann web sites. ∞

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USAGES OF PHYSICS AND THE INVENTOR'S HEALTH

Paulo N. Correa

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Delivered at the Cold Fusion/New Energy Symposium, October 11, 1998, Manchester, New Hampshire.

I will begin by thanking Dr. Mallove for having invited me to speak at this conference and to thank all of you for your attendance.

Eugene selected the title of my address today, but what I have been burning to talk about is not a mere update of the PAGD work but about the politics and the clinics of Physics, the usages of Physics, and the use and abuse of Physics—in the context of our experience with the autogenous PAGD technology. [Editor's Note: See *IE* No. 7, 8, 17]

A Commentary on the Present Politics of Science

For the smashing majority of human beings, science is no concern of theirs. At best if it brings home a little improvement, like a better can opener, a higher resolution VCR, etc., one will find praises for the benefits of science. These days that pass, supposedly of a more sophisticated modernity, abound with popularizations of science. Everybody talks about the fourth dimension, but no one has seen it—no one has seen a Time which is a fourth dimension of Space. Everybody talks about SpaceTime, but no one can verify such an imaginary continuum; what's more, those who peddle this snake oil are the first to admit they lack a Physics of Time. We could go on and on like this about other equally mythological notions—black holes, quarks, the neutrinos, the Big Bang, the collapse of the universe, manifolds with n-dimensions, fractional space, etc. Yet these are accepted physical and mathematical notions, no less than accepted popular ideas. And it is commonplace to hear that the boundary between science and fiction has blurred if not outright dissolved. It even appears as if science is as daring as fiction is—proudly, some go as far as trying to convince us that there was never so much freedom to investigate everything and anything.

But they are sorely wrong—and to the few scientists who have had the chance of seeing something remarkable, something remarkably different, all this modern excitement about science appears to be much ado about nothing. As with all myths, the new fictions open some paths and close others. For a scientist all that matters is whether the paths one opens are factual; if they are fictitious, it is of no interest. Now, what has happened with the modern myths of science is that the paths they have opened lead for the most part to "fictitious facts" (what Nietzsche once called "facta ficta")—and, moreover, they demand that some basic, very basic facts be ignored, be simply discarded, in the name of an irrational belief in Reason. What I'm referring to are basic facts like the existence of simultaneity, facts like actual wave functions, like glaring violations of the Second Law, like the presence of anomalous transmutation pathways—these facts are largely discarded just as official Physics retrenches its fictional accounts of reality into prejudices. And prejudice is arbitrary—it lacks not just rhyme but also any reason.

Take as example one of the critical notions of modern Physics — the notion of a vacuum-state. Many physicists employ the term vacuum state as an equivalent of the Aether, of a plenum that has the properties of a continuum. In the notion of a vacuum state, determinism and nondeterminism would have made peace and thus would have successfully ressurrected the concept of a space filled with energy in the form of a stochastic distribution of electromagnetic waves coexisting with an infinite multiplicity of virtual particles. Yet, modern Physics is unable to offer a single real wave function. There is no recognized actual function of a continuum even for waves, which are, at best, solely understood stochastically as rays. And are we to buy the notion that these are, at bottom, simply the incarnation of probability waves? But not content with this mystification, modern Physics has added the notion and function of virtual particles which, at the very least, would appear to locally violate the Law of the conservation of mass and energy. Even if we are told that this is alright for microscopic interactions, still, what does it mean to create a particle from the void, to actualize a virtual particle, but the notion that *somehow energy would have to come from nothing?*

Yet, when a scientist, an experimentalist or a theoretician, suggests that it is possible that we can get *excess energy from something in that vacuum*, he or she is immediately shunned and branded a fool. Indeed, if a scientist, take Dr. Aspden as example, points out that Newton's rule regarding the equivalence of action and reaction is not a mere corollary for Newton's Third Law of Motion—that it does not require that such equality be satisfied for open systems, or even for closed ones—or if a physicist has the temerity of drawing attention to the fact that the motion of orbital electrons within the atom is virtually perpetual, one is immediately saddled with the epithets of fool or fraud to signify that whatever one says on the matter is an error without interest.

And in which pulpit do these high priests of science and popular science take refuge for their denunciations? In the rhetorical question: How could motion be perpetual when the entropy corollary of the Second Law of Thermodynamics requires the universe to be running down towards a Big Crunch? Everything happens as if Physics is ready to violate the First Law of Conservation of mass and energy, while refusing to accept any violation, local or nonlocal, of the Second Law. It all happens as if only the established scientists, the official scientists in the academia and the scientists officiating in corporate and media endeavors, had a monopolist right to belabor a cosmic error. In the process, any outsider scientists are despoiled of any right to make mistakes and are condemned thereby to eternal banishment for questioning any of the accepted mythical dogmas.

And we can hardly be astonished either that established scientists and would-be philosophers pronounce anathema upon all those who dare violate the Second Law, or search to violate it or claim to have violated it. For them, to violate the Second Law always requires somehow that one also violates the First Law—as if any *perpetuum mobile* required that its energy would have to come from nothing. . . But, you might object to their anathema—what about the electron, does it not have to constantly draw its energy of motion from the vacuum state?—or still you might ask—what about the virtual particles, are they not said to arise from the void and its random energy? To no avail you

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might object, since to postulate perpetual motion in a machine is a dead end street which yesterday was to dare God and today is to dare human Reason, that is, to challenge the Second Law. To put it bluntly, only mad scientists would make such claims.

One of these would-be philosophers summed it up in these sanctified terms, and I quote:

> "But for all of [these inventors and discoverers of perpetuum mobiles] there was the truly Faustian danger of the Devil having a hand in the game, the risk that he was leading them in spirit to that mountain on which he promises all the power of the earth. . . Again and again they succumbed to this ambition; they forced this secret out of God in order themselves to be God. . . And so they created the idea of the machine as a small cosmos obeying the will of man alone. But with that they overpassed the slender border-line where the reverent piety of others saw the beginning of sin, and on it, from Roger Bacon to Giordano Bruno, they came to grief. Ever and ever again, true belief has regarded the machine as of the Devil." (O. Spengler, *The Decline of the West* 1928.)

So the suffering of pioneers and dissenters is hereby blessed as fully deserving! The revulsion these words provoke in any dedicated researcher who is willing to accept responsibility for his or her mistakes simply cannot be described!

For, it is indeed belief, blind belief, in scientific dogmas that have now gained the allure of religion which true scientists dispute and must dispute—in a word, they must dispute the sanity of such beliefs which run counter to the observation of the senses. For after all, there is not even a half-life to the electron—are the electrons then not virtually eternal and perpetually mobile also? But to argue with established scientists and would-be philosophers is a moot point—unless one were planning to hand them on a silver platter the entire body of a New Physics. Even then, they would not even bother to read it, unless the mistakes of an epoch had put them squarely up against a wall.

This, of course, creates a suffocating atmosphere. To the outcast scientist are denied not just free access to the means of investigation and publication, not just the right to commit mistakes, even if intelligent ones, but now is also added an oppressive silence, a silence all around pregnant with the implications of impotence, doubt and contempt.

I have left only a few more introductory words of harshness for those who dare to psychiatrize the challenge of science and the experience of this challenge by a minority of scientists or investigators and inventors—tell us not that we suffer from paranoia or "inventor's diseases," tell us not that we do not know how to market our inventions or discoveries or make them public in that very private way, and above all, tell us not that humanity deserves this knowledge, our effort and sweat, as a Baal Godhead that swallows up its adepts. Do not insult further our intelligence, nor our courage. If humanity is in rags it is not because it has not had enough time, enough history, enough art, enough knowledge, enough science, enough commodities, to make it intelligent, or to improve upon it. There is something sick in humanity, deeply sick, that prevents us all from deriving the best usages that discoveries, inventions, creations, can be put to. It is this sickness that powers the dominant fictions, including those of accepted science.

The problem of the scientist is not a personal drama, but the problem of relating any real science to a society that thinks itself to be the pinnacle of openess but is in fact the greatest jail ever construed on earth. It is a political problem and a medical one, inseparably. If there is no capital flowing into research of excess energy systems, of alternative non-polluting and non-radioactive forms of energy, this is not because capital is wise and weary of investing in foolish dreams. It is solely a political question, in the sense of the politics by which we guide our daily lives. For capital, in the hands of the academics, or the State, or the army, or the corporations, or the common people, never tires of investing in foolish fictions—the fiction of quarks, subquarks and other putons, the fiction of Star Wars, the Bre-X fictions, etc. If there is plenty of money for half-baked ideas, why is there no money even for half-baked excess energy projects? Well, in fact there is some, like the alms one gives to the poor on a bright sunny day. And in fact, it even seems that the precondition to obtain these few alms is to manage to be even less than half-baked. What there is not is money for any serious project. Here I could speak out of knowledge by experience: yes, everyone is interested, the car manufacturers, the telecommunication companies, the Navies, the utilities, religious sects, small time investors, speculators, etc. —but they have one trait in common, to get smart, or rich or powerful over night and to steal whatever they can from the scientist or inventor—his rights, his money, his time, his knowledge, his good disposition even, not to mention, his freedom.

At the end of the whole exercise, not only is there no capital for a serious project, but the inventor or scientist finds himself poorer than before, in all senses of poorer. He is supposed to live on the promises of money and support. Not even to be able to afford legal advice. He is supposed to, each time that he hears the same tale of promises, jump with the delight of a teenager's first date. He is deemed patently stupid by offered proposals that at first glance appear good on paper—but on closer examination contain loopholes wide enough to be forever lost in litigation. And if he is a pioneer uncovering a brand new field, then for sure he must be a dreamer who's easy to fool. Anyway, if he is desperate enough he will have to say yes to all these intolerable forms of slavery, and still some. And at the end of the day, those who say yes to the vampires that pose as having an interest in esoteric science, never had anything to bargain for, not even half-baked.

When it comes to the scientist working on the periphery of the system, on its margins, like myself, where there is no division or department or prestige or grants, then it becomes even worse than for institutional scientists—cat, rat and shoe now demand free demonstrations, proof to the right and to the left, each peddling their pet suggestions: some do not like open demonstrations, they want a black box; others do not like batteries, or don't like oscilloscopes, they want you to use their equipment even when demonstrably it cannot perform the job at hand; others want you to buy data-acquisition systems, but refuse to foot the bill or even to help you do so—while they spend two or three times the same amount on a competition for the year's most ecology-friendly products—and guess what, the prize was awarded to a golf T made of dog poo (yes this happened to us after two years of negotiations with the largest utility in North America); still others want you to couple your technology to theirs, and to make it work for them. Even businessmen, who have not the slightest idea of what it is all about, already have in mind some gadget application—as if it sufficed for them to trot through your laboratory with the air of thousand dollar bills dropping out of their pockets, for their wealth to rub off on you, or for you, somehow, to have miraculously resolved your problem of not having the means to pursue development of your discoveries. And there is no shortage of colleagues and self-styled journalists and investigators who feel they are enti-

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tled to have you demonstrate to them the whole phenomenon free of charge and upon self-invitation, over and over and over. . . After all, as they explicitly put it, don't you need publicity? Maybe, but not like one needs a hole in the head.

The day we woke up to ask for a fee in exchange for our troubles, of demonstrating to so-called interested parties, half our problems disappeared overnight. Yet, those who passed through the screening and wanted to go further—yes, they observed, and confirmed their observation, of the phenomena of the PAGD, its inverter function, the motor effect and the excess energy-scientists from Ontario Hydro, Dr. G. Egely from Hungary, who formerly worked at Brookhaven, top engineers from companies such as Israeli Aircraft Industries, etc.—but notwithstanding the open endorsement by brilliant physicists such as Dr. Aspden, what did we find? The response of a top Detroit executive to the president of GM's European operations, when he presented our proposal, best summarizes it: "Haven't you understood? The EV project [referring to the Impact] is just PR!"

PR that, we might add, paid off, by convincing most of the states of the U.S. to drop their legislative deadlines for the conversion of cars propelled by internal combustion!

But, if you need to know, the most basic bottom line is typified by the following two examples—one from the Alcoa company, which took six months to have their legal team negotiate from scratch another confidential agreement with us only to decline our offer to demonstrate, even free of charge, the technology; then, it returned the two packages of our confidential documentation without those it had assigned to make an evaluation having ever opened them! Or Charter Power Systems, which took a year and half to make an evaluation of the submitted material and data, and then by mistake enclosed *two* internal memoranda addressed to their Director of Battery Engineering and Development in the confidential materials returned to us, the first of which read:

> ". . .Quite frankly, I find myself unable to objectively judge what the documents are saying, probably because my background is in Chemistry rather than Physics. What I do see is a technology that is full of promises yet to be fulfilled; ten years of work which does not appear to have been snapped by anybody in spite of the lofty claims."

And then the second, from another would-be appraiser who tersely wrote: "You can see that [so-and-so] is not convinced of their claims either."

Is this the attention these people are paid for, by their stockholders, to dedicate to their business? And is this the peer review one deserves?

So then, maybe I have a difficult nature, maybe I suffer from inventor's disease, maybe I have made a mistake, but at least I will be able to say it failed not because of our lack of trying. And this, once more, underlines my notion—much disputed by some—that the problem of this field of alternative energy is not one of lacking a ready-made product, or not being able to fire the imagination of people. The problem is a political one, and a problem also of the sheer incomprehension of what is at stake in science, both by the lay people who elect politicians and governing bodies, and by established scientists who dispell judgements without even bothering to attempt to understand the material at hand. Against this state of affairs, all I can say is "Balderdash!" For it is in the hands of the common man to do something about it, and yet the problem is precisely the common man, the little man in each and all of us.

A Bit of Experimental Science: The Autogenous PAGD Technology

Background and Methods

Over a decade ago, Alexandra and I succeeded in isolating a novel regime of plasma discharge, which we termed the autogenously pulsed abnormal glow discharge, or autogenous PAGD for short. This plasma regime involves spontaneous pulsation of a saturated glow discharge by a mechanism of auto-electronic emission. And, as in vacuum arc discharges, the autogenous PAGD deploys anomalous cathode reaction forces—of the type that Dr. Aspden has, for now over forty years, been drawing attention to, and which appear to confirm the substance of his Law of Electrodynamics. The observed pulsation is said to be autogenous because under specific values of diverse physical parameters it occurs (1) with very low applied fields, (2) it is triggered by an auto-electronic emission, and (3) it functions without the need for an external pulse-forming mechanism.

Alexandra and I also designed direct electromotor and inverter applications for the autogenous PAGD regime where a direct coupling between a plasma discharge and specific types of motors is sought. We believe we have also discovered that these anomalous cathode reaction forces are the source of an excess energy contributed by the "vacuum state," that is, by the Plenum, when it is tapped under the various conditions we have isolated and described in great detail. To capture the energy in excess of breakeven, we designed a simple converter circuit which we named the XS NRG™ System. Essentially it employs two battery banks, one at the input and the other at the output of the autogenous PAGD reactor plus associated circuitry. Comparing prolonged resistive battery discharges performed before and after operation of the vacuum tube in the autogenous PAGD regime yielded typical breakeven efficiencies of 5x and greater.

For those of you who want to find out more about the history of the PAGD and our original flndings you should consult: (1) our three U.S. patents—available in their entirety on the web at the IBM patent site; (2) the latest patent granted last November to us by Canada; (3) the essays at our web site; (4) the paper Alexandra and I submitted to the 3rd International Symposium on New Energy in 1996; (5) the metallographic report printed in *Infinite Energy* early this year; (6) the entire report Dr. Aspden wrote on our findings; (7) the *Infinite Energy* issue which Dr. Mallove dedicated to it; and (8) the Mike Carrell articles also published in *IE* (see references).

Over the years we have conducted far more work at our laboratory than we have been able or willing to publish. One of the subjects of my talk today is precisely a summary of the results of one such previously unpublished report describing a manually-operated stand-alone performance of the XS NRG™ conversion system, also referred to as the Ping-Pong control method. The essential idea was to approach a free-running system. Let us begin by introducing:

1) The fundamental circuit of Figure 1 shown here—and corresponding to Figure 9 of the U.S. Energy Conversion System patent (U.S. Energy figures referred to in this article are not shown here, see patent references, Ed.)—as it employs a single reactor and two separate battery packs of unequal size, at the input (drive pack) and at the output (charge pack) of the system, precisely to isolate the two and enable us to measure integral power lost versus integral power gained after PAGD operation of the reactor. These measurements are best performed by long term resistive discharges of both battery packs before and after PAGD production runs. This effectively means separate current and

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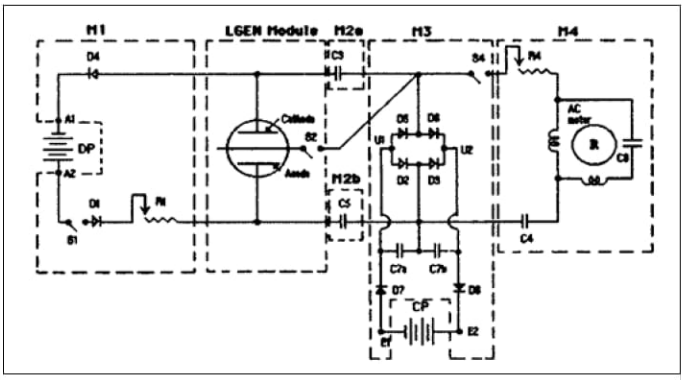

Figure 1. Circuit diagram of the modular experimental XS NRG™ Converter System employing a single autogenous PAGD (LGEN™) reactor and two battery packs, one to drive PAGD production (Drive Pack, DP), the other to collect excess energy (Charge Pack, CP). Excluding the battery packs and the LGEN™ reactor unit, the remainder of the System is composed of four other types of modules: module M1 is the input module, M2a and M2b the filtering and transducing interface modules, M3 the regulator module and M4 the optional motor module.

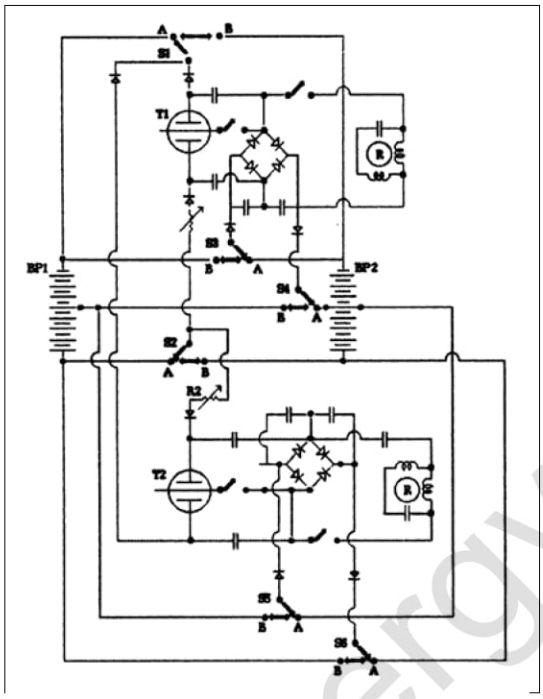

Figure 2. Inport and Outport commutation for a basic XS NRG™ Converter employing two parallel driven LGEN™ reactors with series outputs at the charge pack, and utilizing two identical drive and charge packs (BP1 or BPA, and BP2 or BPB), that are fully interchangeable. Once charged, the second pack (BP2 or BPB composed of two charge packs in series) will function in turn as a drive pack to recharge BP1 or BPA. This defines a full Ping-Pong cycle of battery charging and it can also be accomplished with a single reactor being manually operated to fulfill the distinct functions - in this case, any two PAGD production runs are not simultaneous (for separate reactors), but consecutive (for the same reactor).

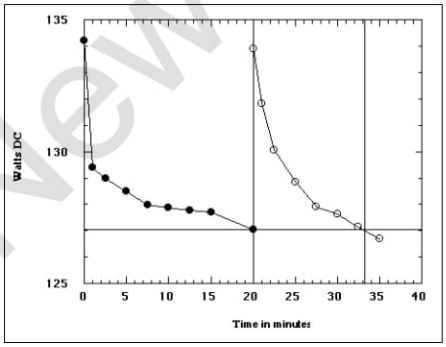

Figure. 3. BPA resistive discharge curves before and after the Ping-Pong PAGD runs (1st pair, Expt 1).

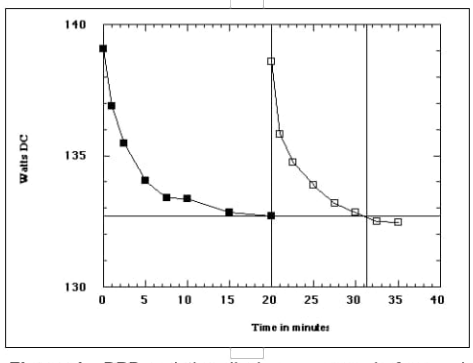

Figure 4. BPB resistive discharge curves before and after the Ping-Pong PAGD runs (2nd pair, Expt 1).

voltage measurements that are integrated over time for separate input and output batteries.

2) In the present work with a Ping-Pong system, the circuit employed is fundamentally that described in Figure 2, or Figure 12 of the cited U.S. Energy Conversion System patent, except that a single reactor is utilized for purposes of minimizing any variance associated with the utilization of different reactors. The experiment consists of employing two battery packs and beginning with one pack as the driver and the other pack subdivided into two charge packs placed in series. The first two PAGD production runs therefore charge the second pack in its totality, with each PAGD run charging one half of the second pack. Final runs three and four have these conditions reversed: the second battery pack now serves as drive pack, to charge, via two other PAGD production runs, the first battery pack which in turn is subdivided into two charge packs. Resistive-measurements of power for both packs are made before each experiment and after a full Ping-Pong cycle of four PAGD runs.

Results

The results presented in this talk were obtained from five separate experiments performed with two identical battery packs, BPA and BPB (46 lead acid gel cells each), without a resistive measurement step before reversal of their functions—as driver or charge packs.

A representative example of the results from one such experiment is shown in Figures 3 and 4. In both graphs, closed symbols refer to resistive discharges performed before the Ping-Pong cycle and open symbols to discharges performed afterward—*i.e.* performed after four consecutive PAGD production runs. If operation of the XS NRG System failed to capture energy in excess of breakeven we should expect the curves obtained for either battery pack after performance of the PAGD runs to exhibit a lower level—or a substantially lower level—than the control level observed before the PAGD runs, respectively below 127 Watts for BPA (last closed circle of Figure 3), and below 132.7 Watts for BPB (last closed square of Figure 4). This is not what is observed. As shown in this pair of figures, after 1.1 hours of Ping-Pong PAGD production per 46-cell pack (*i.e.* after 2.2 hours of overall autogenous PAGD production), both batteries did not lose *but rather gained charge*. Both packs showed a gain of energy consistent with excess energy capture: as indicated by the open circle and open square curves of the final resistive discharges, the observed curves begin well above the respective power level limits, and both take time to reach these limits. All the curves for all five experiments show that both packs gained energy as a function of PAGD production, when compared to the respective power limit levels before PAGD production was undertaken.

This behavior is thus consistent with that observed in a pack that gained energy, such as the typical behavior of a *charge* pack reported in our Energy Conversion System patents and papers. And it is also indicates that both packs are behaving in all respects identically under the same conditions with regard to the shunting of the excess energy captured by the System. The behavior of the packs is symmetric if the energy distribution is a balanced one. This underlines the importance of proper timing, pulse frequency and switchover in this type of experiment, as the inevitable result of performing these experiments without proper control of such parameters is to bias

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charge towards one pack or towards the other.

Results from all five experiments are summarized in the following set of Tables. In Table 1, the net energy and power gains observed for all five experimental pairs are shown expressed as a function of the time it takes to charge a pack of 46 cells by the procedure already described. Net power gained, *which was observed in all cases examined*, is expressed for both BPA and BPB together. Total pulses is the cumulative value of all PAGD events for all the runs of each experimental pair. The fixed, residual argon pressures obtained at the thermocouple are shown on the far right column. As can be seen, the total number of pulses varied ten-fold between 1,000 and 10,000.

Table 1.

Exptl. Pair #	PAGD Time (hours)	Net Gain (Watts)	Net Energy Gain (Wh)	Total Pulses	Pressure (Torr)
1st	1.11	53.25	47.46	1,342	0.09
2nd	1.00	69.94	69.94	1,546	0.1
3rd	1.03	62.9	61.07	2,329	0.15
4th	0.31	32.82	104.22	10,786	0.2
5th	0.87	38.72	44.25	2,558	0.2

Table 2.

Exptl. Pair#	PPS	Net Gain (mWh/pulse)	Net Gain (Joules)
1st	0.18	39.7	143
2nd	0.19	45.2	163
4th	0.31	27.0	97
5th	0.41	15.1	54
3rd	4.70	3.0	11

In Table 2, results are presented in order of increasing pulse frequency (PPS), the average net energy gain per pulse for each experiment being expressed in either milliWatt hours or Joules. It is evident that the net energy gain per pulse increases with decreasing PAGD frequency. This corroborates the data and the correlation presented in our Energy Converter System patents (see for example Table 9 of the cited U.S. patent) with non-Ping-Pong Systems and as obtained by other methods of measurement.

To date, excess energy values as high as 200 to 300 mWh/pulse, that is up to 1000 Joules per pulse, have been obtained at pulse rates on the order of less than 0.05 PPS, in the XS NRG™ Converter circuits. An example is shown in Figure 5 utilizing our data aquisition Superscope™ system:

From left to right are shown current, voltage and power for input and output pulse waveforms. On the lower sequence, the resolution of the current panel on the left is amplified ten-fold on the ordinate to permit us to visualize input current, which peaks at 0.3A. The output current clips for 5 milliseconds well above 100 A (estimated at ca. 135 A), as can be seen from the left panel on the top. The right panel on the lower sequence shows the power waveform with the time resolution of the abscissa amplified ten-fold. Despite the clipping of the output current pulse, the breakeven efficiency measured for the duration of the sample was over twenty-fold in terms of power—46 W in, versus 1kW out.

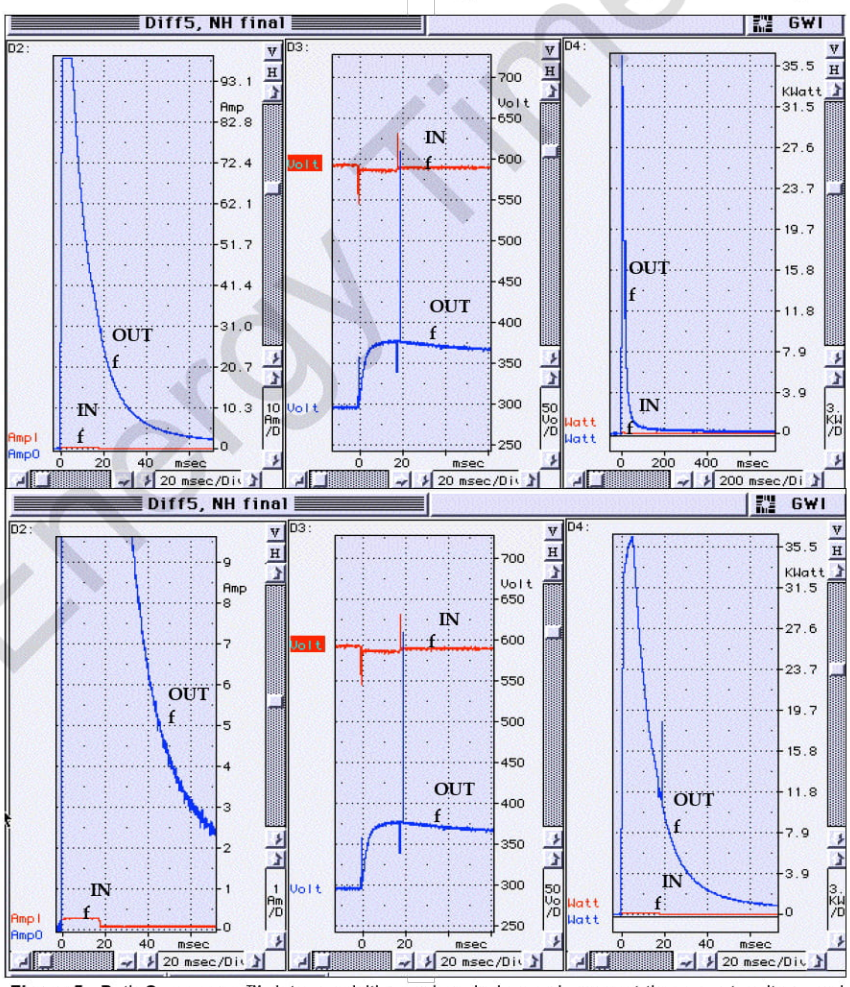

Figure 5. Both Superscope™ data acquisition and analysis panels present the current, voltage and power waveform channels for the same autogenous PAGD event sampled for 0.8 sec at a resolution of 80 microseconds. Output waveform curves are in blue, input waveform curves in red. For the duration of the sample the apparent breakeven efficiency was over 20-fold. DP=46 cells; CP=23 cells. [Colors not reproduced here. Ed.]

Conclusion

The results of these experiments demonstrate that the over-unity condition of greater than breakeven energy return in the XS NRG™ System is a verifiable fact utilizing varied methodologies. And the excess energy values per pulse obtained by integration of power discharges from reliable batteries match the results we have previously reported employing other approaches—namely, the data acquisition system analysis, the oscilloscopic data[1] and the prolonged resistive battery discharges for drive and charge packs not subject to Ping-Pong cycling.

A Note of Caution

At this point a note of caution regarding verification of these facts is in order. After many years of work with the autogenous PAGD and vacuum arc discharges, we have come to the conclusion that auto-electronic emission mechanisms always tap Aether energy. But this coupling with the "vacuum state" is one that can either obey the Second Law or violate it instead, and, in this respect, there is a curious relation between these two alternative outcomes of operation of these types of plasma systems. To begin with, (1) operation of the PAGD/IVAD (interrupted vacuum arc discharge) reactors

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outside of the parameters of area, gap distance, vacuum, residual gas, work-function, current and voltage input, pulse extinction and pulse rates, ranges of the cathode voltage drop, etc., which we have specifled before, is a total and complete nonstarter. Next, (2) utilization of transformer-type couplings consistently yields negative excess energy results, because of inductive conversion of tapped energy in the form of a magnetic reaction of the Aether medium. This may well be what did not happen, but should have, in the power station that Chernestkii is reputed to have destroyed—a balancing of action and reaction. And if this normal safety valve failed it is most likely because, despite magneto-inductive couplings, a situation of resonance might well have taken place there and then. (3) Capacitative isolation of the system fares no better, not just because the tendency for the vacuum to deploy a negative resistance is counteracted, but also because the dynamic capacitance of the tube is interfered with. This is a strange phenomenon, but of direct correlation with the total incapacity to charge a top-notch electroscope, even one with its case grounded, on *certain* days. Furthermore, any placement of a capacitor in parallel with the vacuum gap remits the reactor to operation as a strobotron type device. (4) Another common mistake is a lack of control of the constant heat and power dissipated by the glow phase of the discharge, which is especially promoted by unregulated or current-regulated supplies, inductive couplings, etc. (5) So too is the utilization of relays and vacuum switches in the design of single pack controllers or in the automation of dual pack systems—which introduces vacuum and/or atmospheric arc discharges, coupling of dynamic gap inductances, resistances and capacitances, with all manner of unwanted outcomes.

On the other hand, the positive tapping of energy from the Plenum may take place without this captured excess energy being converted into electric energy. There are four different conversions that happen or are likely to happen under these conditions, and which couple to—as well as may hinder—the electric conversion. Here I will only mention two actions which also appear to be linked and have already made their way to the published literature: (1) One is the conversion of the energy deployed by the anomalous cathode reaction forces into excess heat, as Dr. Aspden explicitly considered in his 1977 British Patent. Essentially, this is the mechanism whereby a vacuum arc discharge which is uninterrupted proceeds, under controlled parameters, to generate excess energy in the form of heat. (2) The other is what Dr. Aspden has called "The Proton Factor" in his 1988 paper, the same factor which, in several of his rather interesting papers and letters, Dr. Paul Rowe has reported. Essentially we are here confronted with the condensation of mass-energy out of the Aether by the action of the discharge.

Epilogue

It is possible that by now the isles are empty of listeners, likely in protest—and that my voice is getting hoarser and harsher. But there is another story I would still like to tell you about, that of one Heinrich Anton Müller, born in 1865 in Boltingen, Switzerland, from humble origins. Having worked in the hillside vineyards of the canton of Vaud, near the Lake of Geneva, Müller invented an ingenious vine trimmer. However, in the process of trying to patent it, his counsel, agents and sponsor colluded to expropriate his invention—and the lucrative exploitation of the vine trimmer was pursued by them and still others. When Müller openly rebelled against this, he was promptly institutionalized in the psychiatric hospital at Münsingen, near Bern, under the standard pretext that he suffered from schizo-paranoid ideas of grandeur and persecution. Seeing himself incarcerated for life for the crime of having invented a machine whose use was of value and profit to others who stole his rights, Müller promised never again to invent another machine which was of use to anybody, in the hope that his machines never again could be stolen, never again be employed for profit, never again be subject to trade and exchange. So, Thevoz writes in his book (Thevoz, M., *Art Brut*, 1976.): "Müller occupied himself with drawings and inventions, the latter with a view to achieving perpetual motion."

This he claimed to have successfully accomplished sometime later (see Figures 6 and 7), in a form that reminds one of the Bessler Wheels (see Collins, J., *Perpetual Motion: An Ancient Mystery Solved?* 1997). His machines, consisting of many wheels engaged in a ceaseless whirl, recall the contemporary efforts of Man Ray to transform useful objects into useless ones. Müller would have succeeded in doing so with a perpetual motion machine, a machine which, by its constant capture of energy, by its constant performance of movement, should be in principle considered the most useful there could ever be. Yet, the notion that such a machine could exist is at once dangerous for our social system and rejected as foolish, as an impossibility. It is dangerous to our social system because there is this common perception that it would make society independent from the oil, coal and nuclear industries—and dangerous because it evokes the notion that if Space is brimming with energy, then power can be had for free. This is also part of Müller's notion—the realization that a perpetual machine ultimately cannot be sold or find a market, even if the inventor is determined to succeed.

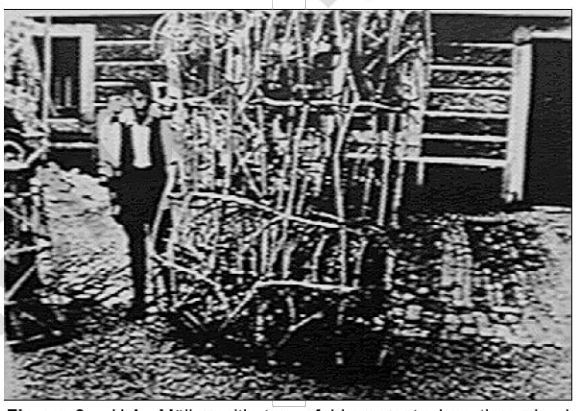

Figure 6. H.A. Müller with two of his perpetual motion wheel machines at the Münsingen Asylum (near Bern, Switzerland, estimated at sometime before 1917).

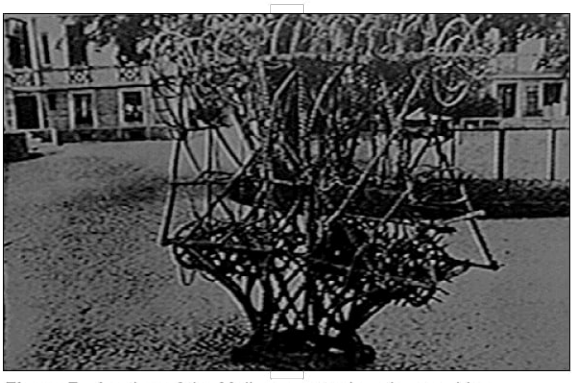

Figure 7. Another of the Müller perpetual motion machines.

But why are such machines rejected as foolish and impossible? Because they raise the specter of getting something from nothing. Yet, consider for a moment what such a machine is—in the words of Thomas Phipps Jr., *it begets something for something, even if unequal, not something from nothing*! And now consider what it stands for in our social world: perpetual motion has been banned to the asylum, to the freak show—where it still finds a way to sell itself—precisely because everybody, the scientist and the common man, persists in believing that it can only equate to getting something from nothing. Here is where we find the

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COMPARISONS FROM THE HISTORY OF TECHNOLOGY

Jed Rothwell

This paper was delivered at the Cold Fusion and New Energy Symposium held in Manchester, New Hampshire on October 11, 1998.

Can an Unpopular Scientific Discovery Be Forgotten?

Earlier today Ed Storms said, "Cold fusion is on life support." Will it survive? Can an unpopular scientific discovery be forgotten? Ed thinks that cold fusion is endangered. In a lecture titled "Cold Fusion—Does It Have a Future?" Nobel Laureate Julian Schwinger said that science itself is at risk:

> The pressure for conformity is enormous. I have experienced it in editors' rejection of submitted papers, based on venomous criticism of anonymous referees. The replacement of impartial reviewing by censorship will be the death of science.

Martin Fleischmann says that society does not want science anymore. People will not accept innovation; it is too disruptive. Some people say progress has stopped. Popular books claim we have reached the end of history, science is close to perfection, there are no more great discoveries waiting to be made, and no more worlds to conquer. John Horgan, a *Scientific American* columnist, wrote a book titled *The End of Science: Facing the Limits of Knowledge in the Twilight of the Scientific Age*. It sounds like a Dave Barry parody, but Horgan means it! These people say that manned space exploration was a bust. We are stuck on earth forever. They say the poor will always be with us, and though we might ameliorate pollution we can never reduce it radically, a thousand-fold. We will muddle through in a future much like the present.

I agree with Thomas Jefferson, who said that scientific knowledge will increase "not *infinitely*, as some have said, but *indefinitely*, and to a term which no one can fix and foresee."[1] We reached the moon only a generation ago; it is too early to turn our backs on the cosmos. Cold fusion together with other new technologies can eliminate air and water pollution, and they can do much, much more. People do not realize how revolutionary it is. Let me give two examples to put you in the right frame of mind, so you will see why we must work to preserve this discovery. Cold fusion can give us more land, and more food.

More land, because it will let us move our industrial plants to the deserts, or underground, or even to the moon if we like. And because we can replace most of our highway traffic with automated air transport. You might wonder how cheap energy can reduce highway traffic. Well, air freight is expensive mainly because of fuel. Cold fusion will make airplanes nearly as cheap as long distance trucks, and new air traffic control computers will allow many more flights, so together they will take most trucks off the highways. That's why I say "cold fusion together with other new technologies" will work with synergy. Today, our highways cover an area the size of Maryland with an asphalt desert. They cost as much as a small war, with similar casualty rates. We do not have to live with this kind of blight and carnage. We can take back most of the land sacrificed to highways—much of it prime real estate—and build houses on it, or return it to nature.

How will cold fusion produce food? Today, most of the cut flowers sold in London, New York and Tokyo are grown in gigantic indoor farms in Holland. In Massachusetts, an automated, one-acre enclosed fish farm in an industrial park produces a million pounds of trout a year. Someday, with cold fusion light and heat, all of our food may be grown in multilevel indoor farms. We could concentrate the farms in an area the size of New York City and grow enough food for every man, woman and child on earth.

Cold fusion will also eliminate most pollution. Eventually, we will make the air and water as clean as it was before the industrial revolution.

The changes I have described will be monumental. They will absorb mankind's creative energy for generations, and give our lives a noble, generous, yet practical and rewarding purpose, something sorely lacking in this self-indulgent age. The transformation may take a century, or longer. Skeptics say it cannot be done. They say we can only hope to muddle through to a future marginally, incrementally better than what we have today. To them I say: open your eyes and look around. Look at the railways, the highways, the great cities, the power and telephone networks. Our parents and grandparents remade the face of the earth and revolutionized our way of life. They did not settle for incremental improvements. In the space of a single lifetime they wiped out most deadly infectious diseases. They went from Kitty Hawk to jets that fly halfway around the world at 900 kilometers per hour carrying 400 passengers. They cut down the vast forests of North America that once allowed a squirrel to travel from Florida to the Great Lakes jumping from branch to branch without ever touching the ground. They bulldozed half the continent. We too can remake the face of the earth. We'll grow the forests back. We can do a better job than our grandparents did by learning from their mistakes, and by using powerful tools like cold fusion and computers. We are not running out of resources. The bounty of the whole solar system is open to us. As Franklin Roosevelt said, "Plenty is at our doorstep, but a generous use of it languishes in the very sight of the supply."[2] We aren't running out of land, or food, or energy, or any natural resource—the only things we are running out of are intelligence and gumption.

Three Discoveries Lost and Found: Genetics, Photography, Superconductivity

Let me get back to the question: Are we really in danger of forgetting cold fusion? Do people ever forget physics? I think cold fusion is vulnerable, because it is not technology yet. It is still science, or proto-science, or you might say it is an uncontrolled laboratory curiosity. We hope to make it into technology. Once people find a practical use for it, survival is assured. A naturalist wanting to preserve an endangered species recently suggested we should raise the animal for human consumption, since people will never let a useful domesticated animal go extinct.

Here are three well-known examples of science that was lost and found: Mendel's genetics, photography, and superconductivity.

Gregor Mendel's papers on genetics were published in 1866, but they were ignored and forgotten. The principles were rediscovered independently by three botanists, who searched the literature, found Mendel's papers, and made them famous after

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34 years in obscurity. This discovery was truly lost.

Photography wasn't forgotten, but it languished for a century. In 1727 German chemist J.H. Schultze made the first shadow picture of stenciled letters, and proved that light, not heat, caused silver nitrate to darken. Primitive photographs were formed in 1777, and in 1802 Thomas Wedgewood made the first good quality photograph, but he was not able to "fix" the image. The image would appear gradually, grow darker, and then fade into black. Only the photographer would see it. Daguerre found a way to fix the images in 1829, and he finally got around to telling people about it ten years later in 1839. As you might expect, another scientist jumped in and claimed priority. At last photographs could be preserved, and passed around in the light of day for people to see. When the news hit, photography was a sensation, but it was also widely denounced as a fraud. A Leipzig newspaper denounced it as an affront to German science and God.[3] The public was surprised by photography, yet people should have known it was coming, because it took such a long time, with slow, step-by-step development. People did not hear about photography because it was a laboratory curiosity literally hidden away in the darkroom. The history of photography provides a perfect illustration of why a demonstration of new technology is so vital.

Superconductivity was discovered in 1911. It languished with no technological application until the mid 1960s when the Josephson effect (1962) was used to make high-sensitivity measurements of currents, voltages, and magnetic fields. High temperature superconductivity was discovered in 1986 but so far it has only been used to replace low temperature devices in established applications. The basic discovery was never forgotten, but it has been marginalized. A businessman might call it a "zombie product"—one that never sells enough to pay but never quite dies either. I think this is the most likely outcome for cold fusion, unless we can turn the situation around.

Have other important discoveries been overlooked? There is no telling. It is like the old joke: "Raise your hand if you are absent." Some people argue that science automatically preserves everything of value. If that is true, scientists are unique; people in other professions constantly misplace important contributions. Great works of art and literature have been lost. Movies are only a century old but already some classics have been lost. Believe it or not, simple programming techniques that were common a generation ago have been unlearned, or temporarily lost. A Windows utility I tested recently took a half-hour to execute. I replaced it with an old-fashioned batch process which runs in two minutes. A letter in *Byte* magazine said, "As much as I enjoy my PCs, I often have the impression that they and their operating systems were designed by young people who somehow missed out on the entire history of computing technology. . . "[4] In engineering, ideas are constantly lost and found. The staff at the University of Manchester built a one-third scale working replica of a 1712 Newcomen steam engine. They gained new respect for Newcomen's original genius. They wrote, "The true functions of the key components were fully understood and their relationship to the operation of the engine appreciated."[5] You might think the first steam engine was simple, but we forgot how it works. I doubt that scientists are better at preserving information than librarians, movie archivists, programmers or engineers, so I suppose important scientific breakthroughs have been lost. Martin Fleischmann says we should comb the back issues of *Nature* for great overlooked ideas.

It may be that science was better at sifting and preserving good ideas in the 19th century, when people started with a clean slate. Every discovery was important, valuable, and easy to understand. Ideas did not compete with an existing canon, because there wasn't much of a canon. Thermodynamics easily pushed aside the caloric theory. Most discoveries quickly led to practical applications. X-rays were put to use in medicine within weeks of Roentgen's announcement. Nowadays, new ideas must compete with viable old ideas, so the new ones may die. NMR (Nuclear Magnetic Resonance) is not compelling when we already have X-rays. People think of cold fusion as another form of nuclear energy, along with fission and hot fusion. They compare it to other pollution-free, zero-fuel energy sources like solar, geothermal, wind or hydroelectric. These alternatives make the development of cold fusion less urgent.

People overlook ideas because they are satisfied with the technology we already have, and satisfaction is the enemy of progress. The newspapers say we are living in an age of unprecedented progress. I think we are in the doldrums. Chris Tinsley used to say that every important technology except the laser was invented before 1950: jet engines, computers, semiconductors, antibiotics, nuclear energy . . . We have been living off the intellectual capital of the past. Personal computer architecture has not changed in 18 years. It was obsolete when it was introduced in 1981, with two serial ports, 640 KB addressability, and ersatz multitasking. We made the PC our standard, we have not changed it, and that decision has cost society billions of dollars in lost productivity and lousy software. Admiral Grace Hopper was a software pioneer and the lead designer of COBOL. During an interview shortly before she died, she listed some innovations she was thinking about. Her ideas would have revolutionized software far more than minor add-on improvements like Microsoft Windows, which programmers call "window dressing," "DOS with a bag on it," and which the *New York Times* called, "an edifice built of bailing wire, chewing gum and prayer."[6] We have become inured to unimaginative, slow, unreliable software and other third-rate consumer goods. We should demand more. Critics say Americans are too self-centered and too demanding. I am glad they are demanding. Software will improve when consumers demand it. Cold fusion will be developed when consumers demand it.

The Generational Role Reversal, and Why Old Coots Are Today's Young Turks

One of the reasons we got into this mess is what I call "the generational role reversal" or, why old coots are today's Young Turks. Max Planck said:

> A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it.[7]

But cold fusion was discovered and confirmed by old scientists and it is attacked by young scientists. I fear the opposite of Planck's progress: a new scientific truth will die as its proponents eventually die, and a new generation will grow up that never heard about it. McKubre said in his closing remarks at ICCF-7:

> As our average age increases with each ICCF, as the rate of progress isn't as fast as it could be, we will just die before we understand this. Our critics will succeed not for any good reason but because we have lost the capacity to stand up at a microphone and speak.

Historically, young scientists have made most new discoveries. They outlived crotchety old naysayers and triumphed in the end. Young scientists were encouraged to think independently and try new ideas. When they failed, no one held it against

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them. They have more energy. They do not realize how difficult the problem is, so they boldly solve it. They are not encumbered by knowledge of present techniques, so they invent new techniques. This is considered the natural order of things. Perhaps it is natural, but it is also a cultural construct. Unfortunately, the culture of science has changed. We punish failure. Research is too formal. Fun in the laboratory is discouraged. Young people have grown up with this system and know no other. Nowadays, old scientists are in the vanguard. They have tenure. They came of age in a more dynamic, liberal era when mistakes and adventures were encouraged, and progress was in full flood. Young scientists are afraid to step out of line. Controversial research will jeopardize their careers. Too much scientific funding is controlled by policymakers in Washington and science journal editors who demand allegiance to established theory. (If I may be permitted a political aside, I see this as a symptom of our politically correct, conformist era, which is also characterized by neo-Victorian, hypocritical, holier-than-thou sexual morality, censorship, and invasion of privacy by government snoops.)

Not only in science, but elsewhere in society the traditional roles of the generations have been reversed. In politics, people over 65 are the free thinking, open-minded liberals. They are the subversives who say, "Why not? Why can't we change things?" The Great Depression and Second World War taught them the world can change overnight and monumental tasks can be accomplished quickly. Freeman Dyson wrote, ". . . [The] experiences of World War II made an indelible impression on people of my generation. At the bottom of our hearts we still believe you can have anything you want in five years if you need it badly enough and if you are prepared to slog your way through the barriers of confusion and incompetence to get it. . . The accepted wisdom says that, no matter what we decide to do about economic problems, we cannot expect to see any substantial results [for 15 years]. The accepted wisdom is no doubt correct, if we continue to play the game by the rules of today. But anyone who lived through World War II knows that the rules can be changed very fast when the necessity arises."[8] Our job is to convince people that the necessity for change has arisen, and the means are at hand.

Why We Need a Demonstration Device

To summarize: we face apathy, loss of faith in the future, establishment opposition, and generational role reversal. These problems would not matter if cold fusion was easy to replicate. But it is hard to replicate, which puts us in a pickle. Gene Mallove and I think that the only way out of this predicament is to make a demonstration device, to convince the public. We need a proof-of-principle demonstration, that is, a miniature version that shows the thing can be done. It could be a jury-rigged prototype; it does not have to be practical. Investors must be wooed first; the scientists will follow. Traditional academic methods of persuasion, such as peer-reviewed journal articles, have been tried and found wanting. If cold fusion had been judged according to the rules, the debate would have been settled nine years ago when Fritz Will and Mike McKubre published. Gene and I tell scientists to put aside their theories, stop trying to improve their calorimeters, stop trying to improve replicability for the time being, and concentrate on a public demonstration instead. A demonstration should be installed near a big city so that investors and scientists can come have a look. We think our laboratory in Bow (NERL) would be the ideal location!

Many scientists respond to our suggestions with hostility, because they do not understand our strategy. They think we are denigrating theory, and belittling the need for rigorous experiments and better calorimeters. We say first things first: get the funding, and then go back to your research. Many scientists dismiss this as grandstanding or a publicity stunt. It is a stunt, and we need stunts! We favor science by press conference.

When Edison was developing the incandescent light, he hung lights outside the Menlo Park laboratory and turned them on every evening. People came from miles around to see. Extra evening trains from New York had to be scheduled to accommodate the crowds. This did not stop the inevitable attacks by the establishment. A distinguished professor called the light bulb "a conspicuous failure, trumpeted as a wonderful success. A fraud upon the public." *Scientific American* published a letter saying it would be, "almost a public calamity if Mr. Edison should employ his great talent on such a puerility."[9] Edison did not sway the establishment at first, but he enlisted broad public support. The hot fusion scientists attacked Pons and Fleischmann for doing "science by press conference" but they themselves depend upon press conferences to impress the Congress and sway public opinion. The cold fusion press conferences in 1989 were a good idea. We should have held more. SRI should hold one today. They should boldly declare they have confirmed cold fusion.

Science and Stock Markets: The Cyclical Nature of History

The generational role reversal is one reason we got into this mess. Another is that science happens to be at a low ebb. It is going through a conservative, uncreative phase in which theory overrules experiments, talented young people ignore science, and experts go around saying this is the twilight of the scientific age. I think that science is cyclical, like the stock market. An upsurge begins when a dramatic improvement comes along. Success follows success, excitement causes more excitement. Society rewards the enterprise with bigger budgets, more money. Eventually the process gets out of hand. Too much success carries the seeds of future failure, breeding smug satisfaction, hubris, bloated budgets in science, and irrational exuberance in the stock market. Giant projects like the hot fusion program and the supercollider are botched; the public loses faith, budgets are cut, morale plunges. We reach a low ebb, a crisis. The crisis leads to self-examination, house-cleaning, renewal, revitalization and the cycle begins again.

Our job is to trigger this revitalization by convincing the public that cold fusion is real. The public will demand that scientists investigate cold fusion. R&D budgets will be allocated, and then the physicists will say they believed it all along. Stanislaw Szpak, a cynical cold fusion researcher in the Naval Research Lab, explains that, "Scientists believe whatever you pay them to believe." The revelations of cold fusion will rejuvenate the rest of physics when people realize the experts are wrong and we do not know everything after all.

I hope we succeed in my lifetime. Regardless, I have faith that science will recover, and mankind's adventures will begin anew. The earth will be healed, and the marvelous bounty I envision—food for everyone, the resources of the whole solar system—will be ours. Science is at a low ebb, but it will recover. We will again see the kind of dynamic era Walter Lord described in his history of the years between 1900 and 1914. I would like to close with a quote from his book:

> The spirit of an era can't be blocked out and measured, but it is there nonetheless. And in these brief, buoyant years it was a spark that somehow gave extra promise to life. By the light of this spark, men and women saw themselves as heroes shaping the world, rather than vic-

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INVESTING IN NEW ENERGY TECHNOLOGIES

Daniel Cavicchio, New Energy Partners

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This is an edited version of a speech given at the Cold Fusion and New Energy Symposium held in Manchester, New Hampshire on October 11, 1998.

Earlier in the morning Gene Mallove made a comment that may have slipped by a number of you. He said that what the field really needs is a business, a product. Publishing papers is great and there have been a number of papers published; the evidence is out there. The critics are going to be critics until there are products out there that are demonstrating over-unity.

New Energy Partners was formed to take this technology from the lab into the marketplace.

The traditional tenets of investing include needing technology, management and money. What we have seen in this field is that the technology is there, albeit sometimes in a lab and sometimes lacking the engineering content. We are trying to bring the money and the management to bear to the task. There are still a lot of challenges to face before we have commercial products, but the basics that we have to work with are there.

At New Energy Partners, we are going to invest in environmentally clean and low-cost technologies that could revolutionize the entire energy industry. We are not looking at solar power or that type of thing. We are looking at revolutionary technologies.

I want to say just a few words about the environment. One of the things that I think the new energy field could do more of is to bring environmentalists into it in a much stronger way. Environmentalists have a lot of energy and fervor, as you know. A number of them have a lot of money as well, so it is important to talk more to them with respect to what this technology can do.

We say we have pollution problems here in the United States, but it pales in comparison to pollution problems overseas. You cannot even breathe in places like Mexico City and India. In Mexico City, people can only drive their cars every other day of the week. The problems in these developing countries are immense. They cannot afford pollution control technologies. So, the things that we are talking and hearing about today have a tremendous opportunity to clean up the world, clean up pollution and provide low-cost energy to everyone. That is part of the fervor that Gene feels, that I feel, that we all feel in getting excited about this field. Not only is it tremendously exciting new technology, it is also doing good for the world and the environment.

Who are we at New Energy Partners? We are a venture capital limited partnership. In that respect, we are like other venture capital firms. We bring in limited partners. In our case, we bring in entities or individuals that are called accredited investors. Basically what that means is that they have a certain net worth which enables us to raise money without going public. The general partner is a company called Energy Horizons, which Gene and I manage. We invest in companies that possess revolutionary energy technologies.

We are business people, so let's talk about business. First and foremost, let's talk about the energy market. Figure 1 shows the market for energy consumption. When we talk about energy consumption, it does not include the market for energy devices. I have been meaning to do some more research on energy devices such as hot water heaters, automobiles and power plants. The market for energy devices is even larger than this market. However, the energy consumption market is a $505 billion market. That figure was from 1994 just in the United States alone. Multiply that number by three or four to get the worldwide market. That is what we consume in energy. The bulk of it is electricity, followed by gasoline (natural and liquid petroleum) and then other products such as diesel fuel. That is the market that we are in the process of displacing in some respects. This market is what economists call very price elastic. If the price goes up, the consumption declines. If the price goes down, the consumption will increase. So, as we introduce products into the market that are going to bite into this—cut it in half or third—the opportunities to use energy are going to explode. There is going to be more and more energy used for things that are marginal at this time. I will come back to the energy market again later.

What are the technologies that New Energy Partners are interested in? First and foremost, we are interested in over-unity devices. I like to use the words "new hydrogen energy." I try to stay away from the term "cold fusion" because investors will roll their eyes. Another term we use in addition to new hydrogen energy is "chemically-assisted nuclear reactions." Cavitation devices is another category. We do not know what drives cavitation devices. It could be new hydrogen energy; it could be something else. There are a number of electromagnetic devices. We talk about tapping the zero point energy. There are energy fields that we believe these electricity devices are capturing. We believe that there are yet-to-be-defined other forces that these devices can harness.

As the fund grows larger, we will also stop to look at most revolutionary energies, but still ones that are on the leading edge, such as fuel cells and the radioactive remediation devices which are related from the technology point of view to the other technologies we deal with. We are not limiting ourselves at all in terms of the categories of devices and we focus more on the empirical results of these devices rather than on the theoretical underpinnings.

Gene has spent a little bit of time on the issue of "is this stuff real?" I'll take just one minute to summarize some of the results

Figure 1

U. S. Energy Market

	$ billion (1994)
Electricity	$201
Gasoline	130
Gas	86
Other Petroleum	82
Coal (Ex-utility)	6
Total	$505

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from researches. There have been thousands of papers published now in these fields. I have picked out the ones that tend to be more credible for investors because a lot of very reputable institutions, research labs and government labs have looked at these studies and confirmed the results.

EPRI/SRI showed excess energy between a few percent and 350%. The Naval Weapons Research Laboratory at China Lake had excess power in 28 out of 94 experiments. *The Journal of Electroanalytical Chemistry*, one of the very prominent peer-reviewed journals, published a paper by Fritz Will *et al.* regarding tritium generation. Shell Research in France has found excess energy. NASA replicated the BlackLight Power cell, showing that it produced excess heat. These data go back quite a number of years—1993, 1994, 1996. The technology has been around. There is no question that this technology is real. That should be irrefutable at this point.

The technology is robust. I like to use ENECO's chart [not shown here] , one of the companies that are doing research in the field and one of the portfolio companies of New Energy Partners, because it shows that the energy effects of new hydrogen energy have been demonstrated in a wide variety of cells with a wide variety of excitations. Across the top is the source of energy that excites, almost like a trigger mechanism, as Peter Graneau was talking about. You can start if off by electrolysis, ionization, mechanical energy and even through pressurization, cavitation or just pure heat. These are all the different researches as of this report. If you use electrolysis, you can make it work with heavy water, light water, molten electrodes, etc. This is not just one science. This is not just one technology. It is very robust. What this says is that there are going to be a number of companies that are successful as time goes on and a number of new sources of energy.

How does New Energy Partners do business? First of all, we do not fund basic research. We are a company that is focused on commercialization. We want to get products on the market. So, first and foremost, we look for companies that have a working prototype, a device demonstrating at least 50% over-unity, hopefully 100-200%. It may not be a commercial prototype; it may just be a lab prototype, but it has to be something that is working that we can check out. Second, we make an engineering and business assessment that the product could result in commercialization within at least two years. That is something that is important to our investors; they want to see revenue being generated in a relatively short period of time. And third, we want to be sure the management team is in place to handle commercialization or, if the technology has been developed by a scientist, that he is willing to license or contribute the product in exchange for equity to a commercialization company.

More often than not, we are finding that the science in this area is basically a lab science and the inventors are scientists, not engineers. We are not going to just say, "Okay, here's $1 million and spend the next few years continuing to do your science." We are going to say, "We like your technology. We think it has commercial merit. We need to bring in a management team for engineering and business management to do what's right by this." I have seen a number of companies and technologies in my field that can whither away the technology because they don't know what to do to make it commercial. It is a totally different skill set to make something commercial, versus developing it in the lab. So, what we do is make companies. We recruit a CEO. Initially that CEO may be more of a technical person. Eventually, as the company builds, they will become more of a business person. It is the rare person like a Bill Gates who can go from one to the other. We provide the initial financing. In these cases, we will probably maintain majority control, but that is subject to negotiation. We are very flexible with respect to the inventor. He can become part of the company or he can continue on his own. We receive licensing fees in order to fund further research. We will do whatever it takes to make it work.

The goal of New Energy Partners is to end up with seven to ten investments, with an average commitment per company of about $1 to $1.5 million. In other words, we are looking for a fund that is going to become about $10 to $15 million—for our first fund. Once we spend that, we will start raising the second fund. Again, the companies should be capable of having a product in two years and going public in three to five years. That is the preferred exit strategy for our investors. The technology must have the potential for serving a large segment of the energy industry. We are not looking for niche products or something that can be sold to R&D labs. We are looking for products that can take over a significant segment of the energy industry. We want to have a company that can start off with an investment of $1 to $5 million and end up with a market capitalization of half a billion or a billion dollars in the future.

We are very fortunate that Gene Mallove and his team have been able to continue to expand. *Infinite Energy* magazine moved out of a nice, but unheated spot to a nice and heated spot. The original facility has now become the lab of *Infinite Energy*, and New Energy Partners is guided by results from this lab (New Energy Research Laboratory, NERL). Gene and his team have developed some very sophisticated calorimetry techniques to evaluate these devices. This is critical to our due diligence. We want to get the device to the lab, so we invite the inventor and inventions to visit the lab. Sometimes we just get the invention, but typically we like to get the inventor(s) there too so they can tweak it and do whatever they have to do. The buck stops here. If it doesn't work, then we don't invest. If it works, then it depends on how well it works. We have had the whole gamut. We have had things that we have spent three or four days on and situations where the inventor totally believes he has something, sends us the data, we look at the data and it looks good. Sometimes we even go to the inventor's lab and do the measurements. We bring it to our lab and it doesn't work. Why doesn't it work? We're still trying to figure that out. We have had situations where it works sometimes or partially. And we have had situations where it worked repeatedly. But the key thing is that we have to make sure it works on our premises with our equipment so that we know there are no bells, whistles or mirrors or anything like that. That is critical to our investigation.

Let's go back to the energy markets, because this is also a critical element of our decision process. Figure 2 shows the energy market blown up a little bit more. I have broken it down into the sector: residential, commercial, industrial and transportation. Most of the electricity market is residential. The rest is commercial and industrial. Gasoline, obviously, is mostly transportation. The key here is that it is a big market and we like to find niches that are still very sizable that we can take our inventions and plug them into. An example of one of those niches is shown in Figure 3, which explodes residential and commercial electricity and gasoline use with respect to hot water heating. There is a $20 billion market for the consumption of hot water heating in the United States. About half the market is electricity, half gas. It is mainly residential, but there is also a good chunk of commercial for food processing and restaurants. Equipment sales for residential hot water heaters are $2.3 billion each year. I am sure all of you have had a hot water heater that has gone bad and you have had to buy anoth-

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er one. In your home, a hot water heater may last five years, but commercial ones seem to last fewer than that. We estimate the commercial market at $1 billion. The market for new energy devices is here. The energy consumed is going to be very small, so consumption is going to decrease and equipment sales are going to grow.

Let's peel the onion one more layer by looking at the commercial hot water heater market. These are bigger units and there is about $400 million in sales each year for just electric hot water heaters. You may ask, "Why do people heat water with electricity? Isn't that more expensive?" Well, it is more expensive than gas, but there are a lot of reasons why people use electricity: equipment is less expensive and sometimes gas is not available in an efficient manner. The fact of the matter is that half of the hot water heaters out there are electric. A commercial electric hot water heater consumes $5,000 a year of electricity and lasts about three to five years. If you had a device that was triggered by electricity and had a 2:1 over-unity, you could save $2,500 a year. At that price, you could probably charge an extra $2,500 for that device. That would give a one-year pay back for the user and that's usually an attractive pay back. Assuming the market went over to this new device, you have created a $250 million market. That's not a bad-sized market for one little niche. With Demand Side Management, a lot of utility companies are funding devices that save electricity and most likely they would fund a good portion of the new device so that the users could save electricity.

A bigger market would be the residential electric hot water heater market, with annual wholesale sales of $559 million. There is smaller energy consumption, so the over-unity device would need to be a lot lower in cost. In the commercial market, we could sell it for $2,500 but in this market we would have to sell it for $100 to $500. You could probably sell towards the higher end, as the consumer market tends to tolerate a little longer pay back. But even if you sell the device for $100, there would be a $440 million market.

My last discussion regarding the markets will show the dynamic change this could bring to the gas hot water heater market. Gas hot water heaters are a little more expensive than electric—$361 versus $127. That is another reason why people use electric. Consumers in warmer climates tend to buy things that have a lower first cost. Developers and builders put in lower first cost units. But the cost for electric ends up being much higher. Gas costs $6.20 per million BTU's, while electricity costs $22.30. Electric units are generally used where there are low hot water demands, electricity costs are low or gas is not available. If over-unity devices enter the market, the cost of electricity to heat hot water will go down by a factor of two or three. Then it will be equal to the gas price and this will begin to put the gas hot water heater market out of business.

Gene and I are looking at a device right now that is a fossil fuel device triggered by gas. It is a gas burner which produces zero pollution and has an over-unity of 200%. So, here's a gas-driven burner that produces three times as much energy as conventional chemistry or heat theories would say you could get out of gas. So, what's going on here? We don't know. We are in the process of testing it out. It's obviously more than burning gas; there's something else going on that is tapping energy from the universe. So, if an over-unity device enters the market, the two technologies can have a fight with one another. The bottom line is that the cost of heating hot water is going to go from $18 million a year down to maybe nothing.

A number of you who have been in the field know that there are a number of devices out there that do nuclear waste remediation. Right now, it is considered to be a $100 billion problem, although that is anyone's guess right now because they are storing it all and creating big salt mines and underground caverns out West. This is not an acceptable solution right now, either politically or economically. A number of these new hydrogen energy cells have demonstrated that they can solve the problem, although the on demand replication is not yet adequate.

A number of our potential investors ask, "Is the timing right?" They accept that this looks like a new technology and is interesting, but they say, "Is it really going to happen within my lifetime? Is it going to happen within the next two years or is it going to happen well after that?" It is tough to answer that. When the stock market has gone up by 30% a year for the last three years and you talk about New Energy Partners having an annual rate of return of 450%, they say, "Well, I can do that in the stock market." So, I don't know whether the market leveling off is going to help or hurt. It will certainly help in terms of the competition, but if there's no money in people's pockets, then that might hurt. Anyway, we have found a significant number of companies that have working prototypes showing over-unity. Replication and repeatability are engineering issues, but we don't want to take that lightly. Some cases are more difficult than others, as there are variables that need to be controlled and aspects of the device that still need engineering work.

As I said earlier, we need money and management. The first product that gets out there that's commercial and can be demonstrated is really going to explode the field. Our goal is to get in on the most promising companies early and be able to

Figure 2 Energy Market

End Use U. S. Energy Expenditures[a]
1994 - $ billion

User Sector	Electricity	Motor Gasoline	Gases [b]	Other Petroleum [c]	Coal/ Other	Total
Residential	$84.6	$0.0	$35.0	$6.3	$0.9	$126.8
Commercial	69.7	0.2	16.5	2.8	0.1	89.3
Industrial	46.3	1.7	34.4	21.0	5.1	108.5
Transportation	0.4	127.9	0.3	51.4	0.0	180.0
Total	$201.0	$129.8	$86.2	$81.5	$6.1	$504.6

Source: U. S. Government Energy Information Administration

(a) Excludes fuel sales to electric utilities, which is primarily coal.
(b) Natural gas and liquefied petroleum gases.
(c) Mainly distillate fuel, jet fuel, residual fuel.

Figure 3 **Hot Water Heating Market**

Energy Consumption

	$ Billion		
	Electricity	Gas	Total
Residential	$8.5	$8.5	$17.0
Commercial	1.7	2.0	3.7
Total	$10.2	$10.5	$20.7

Equipment Sold

	$ Billion
Residential	$2.3
Commercial	1.0 (est.)
Total	$3.3

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make twenty to thirty times our investor's capital—perhaps not on the whole fund, but on the successful investments that is the type of return we are looking for.

You do not have to have a successful product to get good valuation in the market. I use the example of Ballard Power Systems, a Canadian company based in Toronto, that is commercializing hydrogen fuel cells—basically taking the technology that has been developed by the government and trying to drive the cost down. They really are not a commercial company yet. They have a source of revenues, but it tends to be from licensing fees and demonstration projects. Daimler Benz and Ford have invested in this company. But they are not really in the business of making product day in and day out. Yet when they went public in 1995, their market capitalization went up. It was up to $2.5 billion, but with the recent stock market decline has dropped to $1.7 billion. Here is an example of a company that is a pure play in the energy field and can have that kind of valuation before they are really producing product day in and day out. So, I'll say to our potential investors and our investors: we get in the companies, bring the product to market, take the company public and then the investment is liquid. It is a very exciting field.

Gerald Celente introduced me to this field. He wrote *Trends 2000* and is founder of the Trends Research Institute in Rhinebeck, New York. He is a very reputable person who has a lot of Fortune 500 clients. In his book he says that these new technologies "will be the single biggest investment opportunity of the twenty-first century."

Arthur C. Clarke has said, "There is this so-called cold fusion. Which is neither cold nor fusion. There are hundreds of laboratories doing it; they've got patents all over the place. I would say that before the end of this decade, the hand-waving will be over and people will accept that this energy exists." He also said that there are going to be a lot of heads rolling at the Department of Energy.

Throughout my career, I have specialized in business and product development, so I have extensive background and experience in this field. Gene is head of our technology advisory group. In addition to Gene, we have a number of other advisers who work with us depending upon the particular type of technology being investigated. Gene is an expert about the technologies and the personalities involved and is knowledgeable in bringing these technologies to the forefront.

New Energy Partners is still in the process of raising money. We have made two investments so far. We have about four investors and we are talking to a number of big institutions at this particular time and hope to have at least $5 million by the end of the year. If there's anyone who wants to invest or if there are people who have inventions, please let us know.

About the Author

Dan Cavicchio has four technical degrees from Rensselaer Polytechnic Institute and the University of Michigan. The first part of his career was spent at McKinsey and Company, which is a management consulting company that consults to Fortune 500 companies. He worked with the management of technologies group strategic planning and business development. For six years he was the director of business development at American Can Company working on new products and new business. As co-founder of Greenwich Venture Partners, Inc., (1984-present) he has specialized in buying distressed companies that had good technologies but were lacking management and marketing.

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Müller effect, so to speak, where the scientist can only escape by art, and the artist by science—where the machine of the highest utility is useless for the existing social system. It is only fitting that all of Müller's machines were destroyed by order of the hospital management, without a further thought being given to them. After all, what could a paranoid-schizophrenic possibly have to offer?

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tims struggling through it.

Actually, this was nothing unique. People had seen the spark before, would surely do so again. For it can never die as long as men breathe. But sometimes it burns low, leaving men uncertain in the shadows; other times it glows bright, catching the eye with breath-taking visions of the future.[10]

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HERETICAL VIEWS

Thomas E. Phipps, Jr.

908 South Busey Avenue, Urbana, Illinois 61801

Afterthoughts following Dr. Phipps' presentation at the Cold Fusion and New Energy Symposium held in Manchester, New Hampshire on October 11, 1998.

There seems to be little question that science in our time has lost its way. It has degenerated into a set of professions, complete with attendant politicizations (cronyism, logrolling, back-scratching, etc.). On the side of theory, it suffers from the hubris characteristic of any intellectual monopoly. What we call the Establishment in science is actually academia. The latter imposes as its central dogma the inseparability of research and teaching, both of which, of course, belong exclusively to the academy, along with all associated perks—meaning, earthly rewards such as government money, and ego rewards such as prestige, power to exploit the slave labor of graduate students, total control of the next generation's intellectual marketplace, etc. To those outside the charmed circle is denied by definition the right or possibility to do "real science." If a patent office clerk happens to do what is deemed real science, that stray sheep is quietly drawn into the academic fold and the central dogma stands. . . indeed, it stands so much the taller.

Unfortunately, the Lords of the Science Establishment typically display regressive behavioral characteristics that make them unworthy to kiss Faraday's toe. For instance, when scientific bad news is brought to them, their first instinct is to kill the messenger. Hence only the news that is fit to print appears in their first-line journals. Although the resulting barrier to communication is not impenetrable, it is backed by a wall of indifference—a barricade, if you will, against real progress—that truly is impenetrable. Any accidentally printed news that does not fit is universally ignored.

Another regressive characteristic is the suppression of pluralism, particularly in the foundations of science, where it is most needed. Doubtless the need to feel good about oneself at the level of one's basic presuppositions is only human . . . but nowhere in the canons of science is it written that being "only human" benefits the search for truth. Still another, and related, regressive feature is the reliance upon Authority—upon that expert who by definition makes no small mistakes. In the fable a child laughs and the Emperor's parade of folly disintegrates . . . but in real life that does not happen, because the child is in training to be an Emperor.

The fallacy of binding teaching to research lies in the fact that the typical teacher is not prone to learn—the least vigorous seekers for truth being those who already possess it. And why should anyone teach who does not either himself possess the truth or else writhe in secret envy of his fellow Authorities who do? From this a detached observer might conclude that the most gifted (self-assured) teacher has exactly the kind of mentality that makes the worst researcher. So, what is the mentality required of the best researcher? I should say he has primarily a need for all the humility that can be gathered into the human form. For only the humble are driven into action and wholly, purely sanctified by that elusive *sine qua non* we call "scientific curiosity." Not only academia, not only the team spirit of professionalism generally, but everything in modern life, conspires to blight that rare and tender plant, curiosity. It seems that the main gift to the young of what we call civilization is a sense of knowing it all. And who, hampered by such a gift, has the mental stamina to foster—against all the world—a flowering of personal curiosity?

Perhaps even a Faraday, starting out today, would have to spend his otherwise most productive years in unlearning his education. To everybody from Kin Hubbard to Mark Twain has been attributed the observation that, "It ain't the things we don't know that can harm us, it's the things we do know that are wrong." The reason this folksy saying encapsulates enduring wisdom is, of course, that them as don't know are motivated to correct that condition, whereas them as do know are motivated to. . . lecture.

I voice these prejudices because I should like the cold fusion researchers to know that they are not alone; that they are part of a community of sufferers that stretches across the whole spectrum of physics and beyond - and that the suffering is not their fault but is situational. . . that is, it is built into our culture by terms of reference. Any society misplaces its trust and begs to be betrayed that looks for its salvation to higher education, or to government, or to any other institution than that mysterious and lonely agency that lies between the ears of the human individual. In the case of science, the treason is principally that of its academic clerks. Yet if the opportunity were offered not to those clerks but to any others, one feels that the situation would dictate the same outcome.

How many of these vari-colored self-betrayals can human society withstand and still maintain an illusion of monotonic "progress?" Spengler, I believe, predicted that the higher mind of man would simply go to sleep—sated by its accomplishments, such as they might be. The gurus of modern physics second that motion by touting the imminence of a Theory of Everything. To combat this, we need the cosmic equivalent of Comic Relief. ∞

HERETICAL VERITIES

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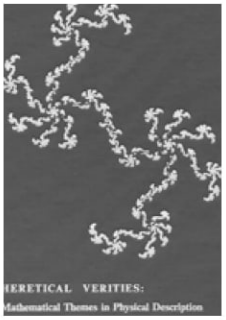

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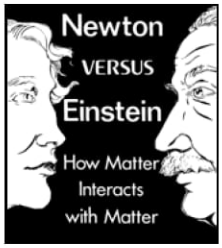

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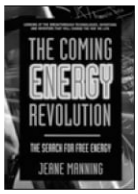

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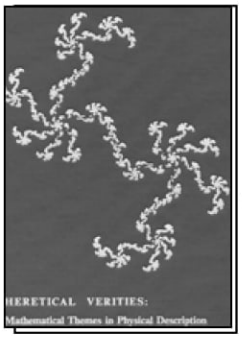

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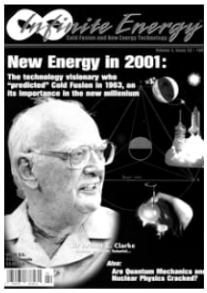

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BlackLight Power, Inc. to Reveal *"Revolutionary Breakthrough"* January 1999

BlackLight Power, Inc. of Malvern, Pennsylvania is keeping everyone in suspense, but the company has placed a prominent notice on its website about a "revolutionary breakthrough to be announced in January."

Will the "January surprise" be a new advance in a demonstration technology of a self-sustaining hydrogen heat reaction? Will it be a prototype commercial product—perhaps even one that generates electricity? Or maybe it will be a clear-cut scientific demonstration of what Dr. Randell Mills believes to be below normal ground state hydrogen atoms—hydrinos, which release u.v. electromagnetic energy, he says, via a catalytic process? (See interview with Dr. Mills in *IE* No. 17) We wait with baited breath, and hope very much that BlackLight Power, Inc. will not disappoint us in January.

Other news about the company posted on www.blacklightpower.com concerns the company's prospective move to the Princeton, New Jersey area. That ought to scare the hot fusion tokamak boys at Princeton's Plasma Physics Laboratory! It would not surprise us if some of the more adventurous PPPL boys and girls jumped ship and applied for positions at BlackLight Power. As a public service and part of this news item, here is a portion of the job posting:

Job Opportunities with BlackLight Power

BlackLight Power, Inc. (BLP, Inc.) is currently experiencing a period of transformation and expansion. As the Company continues to refine its hydrogen-based energy technology, it progresses towards eventual commercialization of Dr. Mills' inventions.

In an attempt to fulfill this vision of commercialization, BlackLight Power, Inc. attempts to create a working environment that is stimulating, but also conducive to productivity. In turn, employees are encouraged to work as a member of a selected research team, while simultaneously maintaining individual projects.

Regardless of the position desired, the ideal candidates for employment at BlackLight Power must possess effective organization, communication and trouble-shooting skills. Self-motivation, a strong work ethic and the desire to contribute to the overall success of the Company are also required.

Additionally, applicants should be willing to relocate as a condition of employment. To accommodate the rapid growth in both the number of employees and projects, BLP, Inc. is currently in the process of relocating to a larger facility in the Princeton, New Jersey area.

In exchange for the qualifications outlined above and in each of the following job opportunities, BlackLight Power, Inc. offers employees an extensive benefits package that includes competitive compensation, comprehensive medical coverage and employee stock options.

Qualified applicants for all BLP, Inc. employment opportunities are encouraged to send their resumes with salary requirements and references to: Amy Cain, Human Resources Coordinator, BlackLight Power, Inc., Great Valley Corporate Center, 41 Great Valley Parkway, Malvern, PA 19355; FAX: (610) 651-4940.

For more information contact Amy: staff@blacklightpower.com

The company says it is trying to fill these positions: Analytical and Inorganic Chemists; Analytical Instrumentation Specialists; Stock Room Controller/Chemical Hygiene and Safety Coordinator; Executive Assistant; Research Scientists; Surface Scientists/Materials Scientists.

U.S. Funding of ITER Hot Fusion Reactor Killed by Congress

The United States Congress has rejected further funding for the International Thermonuclear Experimental Reactor (ITER), which had been an on-going hot fusion R&D program involving the U.S., Japan, Europe, and Russia. This move may lead to the eventual death of that project, if the remaining international partners do not continue their financial support.

As reported in *Science* magazine (October 9, 1998) and elsewhere, the U.S. Department of Energy recalled its 36 hot fusion scientists from their locations in Germany and Japan where they had been working for three years on the design phase of the ill-fated $10 billion tokamak reactor. They were to be back in the U.S. by November 16.

Dr. Ronald Parker, the former head of the MIT Plasma Fusion Center, had been stationed in Garching, Germany as part of the ITER project. *Science* magazine quoted his dismay at the abandonment of ITER: "[the cutback] has created a pretty depressed mood here." And Parker characterized Congress's decision: "completely destructive. But at some point you have to put disappointment behind you and move on to new challenges." This is poetic justice, perhaps, because Parker was among the vanguard of scientists who in the spring of 1989 attacked the work of cold fusion scientists Drs. Martin Fleischmann and Stanley Pons. Parker had called the cold fusion work "scientific shlock" and "possible fraud." Since 1989, the U.S. tokamak hot fusion program at MIT, Princeton, and elsewhere, has received over $2 billion dollars in Federal funding.

Science also reported that DoE's fusion energy work was shifted from its "politically exposed position as a separate budget item" into a newly established Office of Science that deals with energy research programs. Tokamak fusion now receives about $225 million per year. (Estimates circulated that abandoning U.S. participation in ITER would result in $12 million in costs just to close U.S. work on ITER down.)

In a sneaky maneuver a few weeks before the ax fell on ITER, the new U.S. Secretary of Energy, Bill Richardson, signed a "statement of intent" in Vienna , Austria that supposedly would have allowed the U.S. to continue work on ITER until July 1999.

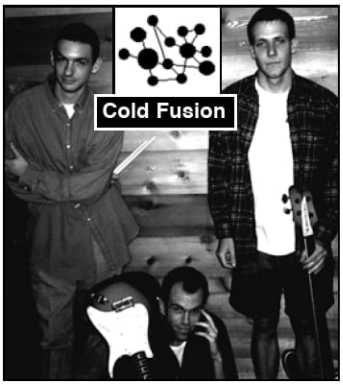
James Sensenbrenner, Chairman of the U.S. House Science Committee, was reported to be outraged by Richardson's "irresponsible" move. He issued a statement: "The project has failed and it's time to move forward. It defies common sense that the United States should agree to continue to participate in a dead-end project that continues to waste American taxpayer's dollars." Even though the statement signed by Richardson has no legal effect, the leadership of the U.S. hot fusion community continues to use it to encourage the notion that the U.S. might re-engage itself in ITER work. Dr. Anne Davies was quoted in *Nature* (October 1, 1998): "I'm very, very pleased. . . This gives us a way forward."

New Energy Times Archive

Cold Fusion Rocks On!

Infinite Energy is always pleased to see groups and companies with products that make use of "cold fusion" for a positive, eye-catching label. In *IE* No. 21 we took note of the software product, "Cold Fusion." Later, we became aware of a rock group in Iowa that goes by the name "Cold Fusion." We have yet to hear its music, but the band has promised us a demo tape soon. For now, this is what the musicians say about themselves—as you can see, they have a "cool" logo too:

Name: Cold Fusion
Music: Modern Rock/Blues
Hometown: Quad Cities, Iowa
Together Since: June 1997



"Cold Fusion is a rock trio that originated in June of 1997. Colin Powers, Ted Gimbel, and Grant McGuire decided to pool their musical talents after attending the same high school, Assumption High, in Davenport, Iowa.

"Currently, all three members of the band are studying in college, but the music lives on. Although college keeps the three busy, they still find time to write music during the school year, and play together during the summer and any extra chance they get.

"Influences on the band include such groups and people as: Eric Clapton, The Beatles, B.B. King, R.E.M., Alice in Chains, Stone Temple Pilots, Buddy Guy, Nirvana, and Pearl Jam. A mixed group no doubt. Cold Fusion takes the variety of sounds and styles and molds them to create their own unique blend of rock and blues.

"Ted Gimbel is the band's lead guitarist and back-up vocalist. He is currently studying at the University of Iowa. Grant McGuire is the band's bass player and backup vocalist. He is currently studying linguistics at the University of Illinois, Urbana-Champaign. Colin Powers is the rhythm guitarist, percussionist, and vocalist. He is currently studying microbiology at the University of Iowa.

"The band has set a tentative release date of January 1, 1999 for a self-produced EP. Current plans only include a distribution among family and friends due to production costs.

"Cold Fusion has high hopes for the future, and looks forward to increased support and energy in their drive to make music."

Obituary for Dr. Hans A. Nieper

Ed. Note: *The following obituary for a noted figure in "vacuum field energy" was provided by our colleague Wolfram Bahmann (Vice-President and Secretary of The German Association for Vacuum Field Energy Mechernich, Germany dvs@gptec.com) and his associate Adolph Schneider. Parts of this obituary appeared in the (German) New Energy Technology journal.—EFM*

The famous physician (oncology) and honorary president of The German Association for Vacuum Field Energy passed away October 21, 1998—three days after his last presentation during a conference on New Energy Technology in Switzerland.

Dr. Hans A. Nieper celebrated his 70th birthday on May 23, 1998. He had been the DVS (German Association for Vacuum Field Energy) honorary president since November 1997. Three days after his last official presentation during the conference "New Progress in Technology and Science" in Egerkingen, Switzerland, he died after what was a second stroke. For many of his friends worldwide, his death is difficult to face.

Just the day before his death, he dictated a letter to the conference organizer, expressing his great enthusiasm for the activities which he had heard about during the Congress. He wrote of his appreciation for the cooperation with the DVS, which was set up in November 1997. (The German-language NET-journal of Jupiter Publishing House has been part of the DVS membership information since that time.)

Beginning during his university years in the early 1950s and more intensively during the 1970s, Nieper was interested in gravitational shielding theories. He was convinced of the possibility of extracting energy from the gravity field or from a more basic underlying energy. He even presented his ideas to the space committee of the United States Senate in May 1973. At the suggestion of physicist Dr. Ernst Stuhlinger, who at that time worked with with Dr. Wernher von Braun in Huntsville, Alabama, Nieper's Shielding Theory of Gravity (proposed in 1953) was linked to the tachyon theory of physicist Gerald Feinberg.

From the mid-seventies to the eighties, Nieper communicated with numerous people in many fields of interest: subtle energy fields, electromagnetic influences on biological systems, aether theories, UFO phenomena, and gravity theories of Seike, Koy, Holt, Koppitz, Marinov, Magyari, etc.

Nieper assumed that the Earth was embedded in a huge cosmic energy field, which he named the "perisolar cushion field." On its face, it corresponded to the "aether" of the last century. In this context, his claims were based on the papers of Nikola Tesla, among others. Similar to the neutrino theory of Prof. Dr. K. Meyl, the interceptive energy absorption creates a temperature rise in the inner core of the planet, proportional to its diameter multiplied by its mean density. Larger planets, such as Jupiter, emit up to three to four times as much energy as they absorb from the Sun. The factor, according to the forementioned formula, is 2.7 times larger for Jupiter than the ratio for Earth. Nieper's theory also gives plausible explanations for the phenomenon of Earth radiations detectable normally only by dowsers.

Dr. Nieper initiated and was a leader of several technical conferences on energy, which began in November 1980 in Hanover, Germany. The symposia were aimed particularly at the practicable possibilities for the conversion of gravitational field energy into electrical energy—alternatives to present-day nuclear technologies. Nieper's worldwide contacts as a maverick in alternative medical treatment were also useful in his DVS work.

The period of the eighties will be remembered as a time of the break-up of old ideas, yet many devices that were shown and claimed to run on space energy did not fulfill their promises. The goal of an evidently self-sustaining device driven by space energy continues as a goal for the DVS today.

In April 1981, the DVS was founded and headed by Dr. Nieper. After more than 15 years as President, in November 1997 for reasons of health, he was succeeded by Prof. Dr. Josef Gruber.

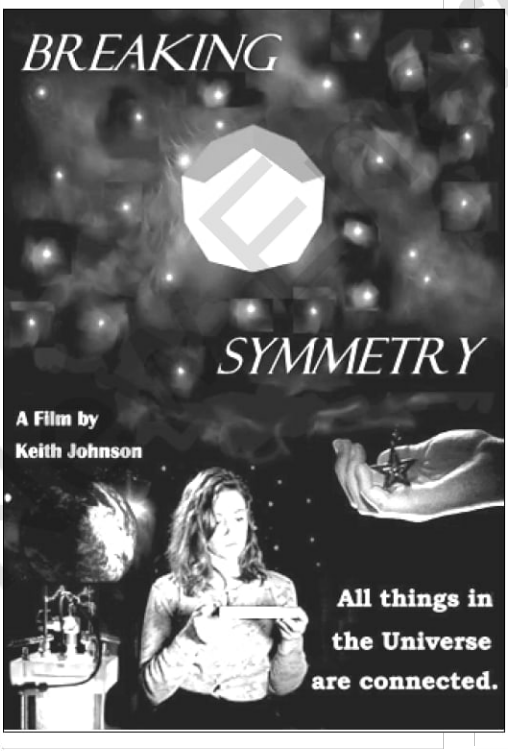
In his book, *Revolution in Technology, Medicine, and Society*, Nieper attempted to spread the knowledge and the demand for the distribution of environmentally friendly technologies. His battle against platinum-bearing catalytic converters was only one of many. In his last letter to the DVS secretary, he emphasized that introducing a new energy technology was the answer to ending the nuclear age, at least in Germany. His biography, *Curious Man: The Life and Work of Dr. Hans Nieper*, was just released in December 1998.

New Energy Times Online

Cold Fusion Movie Poster Created

In our last issue (*IE* No. 22), we noted that MIT Professor Keith Johnson's cold fusion movie, *Breaking Symmetry*, would debut in 1999. Our latest information is that it will, indeed, be shown at the Cannes film festival in Europe in May 1999 and then it will make its way to "art theaters" in the United States. The film is now in final form with a newly refreshed musical score. The film may become a hot item in some of the avant garde Boston and Cambridge, Massachusetts theaters. Then it may make its way into more mainstream distribution. Perhaps a venue will even be found for showing the movie on the MIT campus.

Prof. Johnson has sent us a color rendition of the promotional poster for *Breaking Symmetry*. It appears in black and white here. Those *Infinite Energy* readers who would like one or more of these fine color posters should contact us at *Infinite Energy* (staff@infinite-energy.com or 603-228-4516). Based on the number of inquiries, we will have batches of these printed up and will offer them for sale. They will have many uses: home or dorm room decorations; chemistry, mathematics, or physics lecture room adornment; presents for that special cold fusion critic in your life; and general stirring-the-pot activities on behalf of cold fusion.



Another Award for *Free Energy: The Race to Zero Point (Video)*

The Aurora Awards, based in Salt Lake City, Utah has bestowed its Gold Award in the category of Science & The Environment for the documentary feature, *Free Energy: The Race to Zero Point.* Produced and directed by Christopher Toussaint, owner of Free Spirit Productions in Valley Village, California, the 110-minute program about inventors and their devices which produce non-polluting excess energy has won its third award since being released in 1997. Distributed by Lightworks Audio & Video and available through *Infinite Energy* magazine, the program features visionary scientists and engineers who are discovering new ways of reinterpreting the laws of physics to allow for the tapping of zero-point fluctuations in the vacuum of space. *Free Energy* had already been awarded a Telly Finalist Award and a Profit Producer's Award of Excellence.

With guidance from the staff of *Infinite Energy* magazine, and with a significant grant to Cold Fusion Technology, Inc. by an anonymous benefactor, Toussaint is currently producing and directing a similar documentary called *Cold Fusion: Fire From Water.* It will be available exclusively from Cold Fusion Technology, Inc. for commercial broadcast and also on VHS video cassette for consumers within the first quarter of 1999. The documentary focuses on the field of low energy induced nuclear reactions in solids that has followed from the 1989 discovery by Drs. Fleischmann and Pons.

Dr. Jacques Benveniste Wins Libel Suit

In a decision by a Paris civil trial court dated September 16, 1998, Excelsior, publisher of *Science et Vie*, a French monthly science magazine, was convicted of libeling Dr. Jacques Benveniste. A money award was made. The August, 1997, issue of *Science et Vie* contained the following statement: "He [the American magician, James Randi] has several major hunting trophies on his wall: unmasking Uri Geller's methods of cheating, and the memory-of-water fraud (see *Science et Vie*, April, 1997)." The court decision specifies that the defendant (*Science et Vie*) produced no evidence that could establish its good faith.

Comments from Jacques Benveniste:

"Furthermore, *The Times* of London, which echoed the same statement, attributed to James Randi, in its September 9, 1998, issue, published a correction with an apology on September 25, 1998.

"It should be mentioned that James Randi himself, in a letter posted in March 1998 to his Internet discussion group, denied having made any allegation of fraud in the memory-of-water case: 'If they (two French Nobel Prize winners) did indeed think that he was a fraud, I disagree.'

"These events cast a harsh light on the inconsistent behavior of French Nobel Prize winners, Charpak and Jacob, who declared, in a January, 1997, article in the French daily, *Le Monde*, that a fraud indeed existed. In fact, if the two Nobel winners had had the slightest bit of evidence pointing to fraud, *Science et Vie* certainly would have produced it in court!

"To conclude, as far as science is concerned, the fact that the results of this research cannot be attributed to either a fraud or an artifact (for which no one in the past ten years has proposed a credible hypothesis) strongly suggests collective behavior aimed at keeping the lid of strict orthodoxy on biological research. On this subject, see the article in the June, 1998, issue of *La Recherche* and my answer in the September, 1998, issue. Also visit http://www.digibio.com."

The above news item courtesy: OBRL News is a product of the non-profit Orgone Biophysical Research Lab Greensprings Research and Educational Center, P.O. Box 1148, Ashland, Oregon 97520 USA

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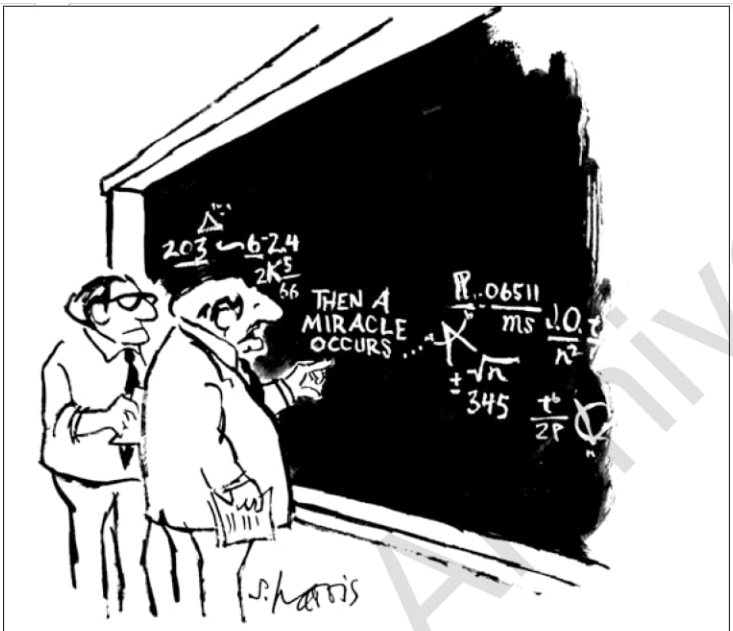
Editor's Note:
In a future issue of *Infinite Energy*, we intend to have extensive coverage of the "Memory of Water" controversy.—*EFM*

Cold Fusion Comix

— A variety of humorous insights about the serious matter of New Energy, New Physics, and Scientific Heresy



"I think you should be more explicit here in step two."

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BAR STOOL SCIENCE No. 4
THE FIRST LAGER-LOUTS

BY SUSAN SEDDON

Extract from insurance claim submitted by Maurice LeClerc, proprietor of hostelry "Le Cochon Noir," Calais, France, April 17, 1620: "It was nine of the clock when two travellers entered my establishment. They appeared a little the worse for imbibing. One, an Englishman calling himself Frank Bacon, claimed to have been on a 'booze-cruise' in order to procure French wines at duty-free prices and had issued the last ferry home. The other, a German named Joe Kepler, was more vague about his reasons for being in Calais, merely murmuring obscurities about persecution and continually enquiring whether anyone had yet founded a military organization which he could join to enable him 'to forget.' I sensed trouble was afoot. The two indulged freely in rough cognac, monopolized my billiards table and argued constantly. The German persisted in expounding what he called his theories of planetary motion as they related to performing trick shots. The Englishman became very irate and counter-argued that balls on a flat billiards table could not be induced to uniformly describe an elliptical path. A great deal of money was lost by the Englishman in wagering against the skills of the German, who was clearly an accomplished hustler. The Englishman grew more and more excitable with each lost wager, particularly so when his funds became exhausted and the German refused to accept his marker. He accused the German of being unscientific in his generalizations based on one set of observations alone. The German, in turn, accused him of welshing on his debts and said that perhaps the only way to convince him of the validity of anything was by means of bribery, the ready acceptance of which, he had heard, being something to which the Englishman was not averse. Everyone in the place fell silent. The two men glared at each other across the table, billiard cues held menacingly aloft. The Englishman eventually broke the silence by announcing loudly that the German would have more success in gaining acceptance for his speculative theories if he persuaded his mother to cast one of her spells. Then all hell broke lose. I estimate the cost of rebuilding my entire bar-room and procuring a replacement billiards table to be in the order of several thousands of Francs."

Next issue: Thomas Edison's Laser Disco Party

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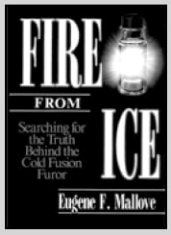

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By Eugene F. Mallove, Sc.D.
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CSICOP: "Science Cops" at War with Cold Fusion

by Eugene Mallove. Sc.D.

The collective wisdom of the so-called Committee for the Scientific Investigation of Claims of the Paranormal (CSICOP) is that cold fusion—and any other claim about an anomalous energy source not taught in the sacred halls of academe—is scientific heresy worthy of mockery and rebuke. That is the message, by commission and omission, that is conveyed in CSICOP's ritualistic debunking of cold fusion and related low-energy transmutation discoveries. (CSICOP has disdain for any other novel energy claims that may result, for example, from Zero Point Energy (ZPE) tapping, but the organization does not spend much time on these pejoratively labeled "perpetual motion machines.") Since the cold fusion announcement in 1989, CSICOP's main thrust in denying new energy sources has been to attack cold fusion research. The abuse occurs in the organization's magazine and in public pronouncements by *some* of CSICOP's dozens of illustrious Fellows. The Fellows include five Nobel laureates, biologist-author Steven Jay Gould, the late Carl Sagan, and Steve Allen, who is listed as a "comedian, author, composer, pianist." Only a small number of the Fellows are known to have attacked cold fusion. Carl Sagan, to his credit, was relatively open-minded and polite compared to others at CSICOP (see *Infinite Energy*, issue No. 20).

CSICOP, founded in 1976, is a non-profit organization of several tens of thousands of individuals. It publishes the magazine *Skeptical Inquirer: The Magazine for Science and Reason*. This glossy, well-financed journal is now a bi-monthly publication; before 1995 it was quarterly. Its current subscriber list is said by CSICOP to be about 50,000 in 80 countries, and it is on newsstands. It is undoubtedly very influential with the general media.

The current financial appeal by CSICOP to raise $10 million for its "Fund for the Future" states that CSICOP ". . .is universally acknowledged as the media's foremost source of objective, scientific expertise on paranormal matters. More than 35 autonomous local groups amplify the skeptical message across the U.S., along with more than 40 autonomous national skeptic groups abroad, publishing some 19 magazines and journals in nine languages."

CSICOP's world headquarters is in Amherst, New York where it has a facility dubbed "Center for Inquiry-International." CSICOP is affiliated with the Council for Secular Humanism, publisher of *Free Inquiry* magazine—a journal that specializes in criticizing religions (organized and otherwise) and mystical beliefs in general—what CSICOP founder Paul Kurtz calls "the transcendental temptation." The Center for Inquiry "Fund for the Future" is a joint venture by CSICOP and Council for Secular Humanism to raise $20 million in endowment funds—$10 million of that intended for CSICOP.

CSICOP's purpose is succinctly stated on *Skeptical Inquirer's* cover: "The Committee for the Scientific Investigation of Claims of the Paranormal encourages the critical investigation of paranormal and fringe-science claims from a responsible, scientific point of view and disseminates factual information about the results of such inquiries to the scientific community, the media, and the public. It also promotes science and scientific inquiry, critical thinking, science education, and the use of reason in examining important issues. To carry out these objectives the Committee: sponsors publications; conducts public outreach efforts; maintains an international network of people and groups interested in critically examining paranormal, fringe science, and other claims, and in contributing to consumer education; encourages research by objective and impartial inquiry in areas where it is needed; convenes conferences and meetings; conducts educational programs at all age levels; does not reject claims on *a priori* grounds, antecedent to inquiry, but examines them objectively and carefully."

All this sounds very high-minded and fair, a policy with which any good scientist might agree. That is, if CSICOP and its journal were really faithful to these governing principles. Unfortunately, virtually from the outset in 1976, CSICOP became a politicized debunking organization. Its tiresome agenda is to assault, often with a barrage of often bogus or flawed "studies" and mockery, any and all scientific heresies that do not adhere to currently accepted theories of physics, chemistry, and medicine.

Of all the claimed non-scientific heresies that CSICOP rails against, several occupy most of the *Skeptical Inquirer's* fire-power. Its favorite whipping boys are: alternative medicine; UFO sightings and alien abduction claims; all paranormal claims involving extrasensory perception and psychokinesis; astrology; "creation science" and, for

On the cover of the *Skeptical Inquirer,* Vol. Xii, No. 4 (this page): From left to right, Kendrick Frazier, Philip J. Klass, Wu Xiaoping, James Alcock, James Randi, Barry Karr, and Paul Kurtz.

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good measure, the activities of any and all religious or spiritual groups—especially those outside "mainstream" organized religion.

In the early 1980s, when out of curiosity I began subscribing to *The Skeptical Inquirer*, I was inclined to take at face value some of the magazine's articles on investigations of paranormal claims and "fringe science" claims. Some of these "investigations" I already recognized were obvious hatchet jobs by individuals bent on dismissing *all* evidence based on "this cannot be" paradigms, rather than on objective examination of evidence. For example, the cavalier, arrogant rejection by Philip J. Klass of the *entire* body of UFO evidence.

Later, when the cold fusion crisis emerged in 1989 and I witnessed first hand at MIT the bigotry and anti-scientific hysteria of the opponents of cold fusion investigation, I was taken aback by wanton disregard of the then growing body of experimental evidence supporting the emerging phenomenon. Even though the microphysical explanation then (and now) is a matter of disagreement among cold fusion scientists, the evidence for nuclear-scale excess energy production and various forms of nuclear ash is now very well established in a variety of hydrogen-metal systems. This has been true at least since 1991. But displaying its true colors, CSICOP early on joined the chorus of knee-jerk cold fusion critics.

I still examine each article in *SI* on a case-by-case basis. But overall my view is that CSICOP, as an organization, could do much to increase its intellectual credibility. In effect, CSICOP appears to be an organized religion for the paradigm-paralyzed. These are philosophers, journalists, and so-called scientists who are so convinced of the solidity of currently accepted paradigms in physics, that they truly cannot tolerate the idea that radical modifications to physical theory might be necessary to account for new experimental findings. CSICOP is theory-driven, not experiment driven—its protests for being so-characterized notwithstanding.

It might be considered natural for the "science cops" of CSICOP to feel comfortable debunking the statistical results of parapsychology experiments or mocking unusual religious viewpoints. But it boggles the mind that CSICOP would have the audacity to conduct warfare against a hard physical science area—cold fusion—for which there currently exist numerous scientific papers in the peer-reviewed and non-peer-reviewed literature.

It is remarkable and revealing that CSICOP uses the term "fringe science" at all in its statement of purpose. Any good student of the history of science must realize that virtually all major scientific discoveries were initially "fringe science." Anomalies and radical scientific ideas that have turned out to be valid, have routinely been ridiculed before their acceptance by established science. I presume CSICOP considers cold fusion to be "fringe science."

For the purposes of this column, let us set aside CSICOP's dismissive treatment of alternative medicine, UFO claims, ESP, and the like, and focus on an area with which we at *Infinite Energy* claim considerable expertise—cold fusion. Perhaps by seeing how improperly CSICOP treats this area, one might be concerned that the organization's handling of other heretical topics could be similarly distorted.

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In the case of cold fusion and related energy and transmutation claims, it is absolutely clear that CSICOP's lack of scientific investigation constitutes intellectual fraud. CSICOP members who do not consider themselves mere propagandists against cold fusion, as some of CSICOP's better known members have demonstrably been, should have second thoughts about CSICOP's editorial position.

In the very latest issue of *Skeptical Inquirer* (January/February 1999, Vol. 23, No. 1), we find a typical unsupported attack against cold fusion. It is buried in a lengthy statement about "Science and Pseudoscience" by a leading Russian organizer, physicist Sergei Kapitza (Vice President of the Russian Academy of Natural Science). *Skeptical Inquirer's* editor, journalist Kendrick Frazier, wrote—or let pass—this introduction to Kapitza's statement: "With the collapse of the Soviet Union and subsequent profound economic crisis, science in Russia is in a difficult state. The rampant social disruption has been accompanied by a veritable flood of pseudoscience.

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The rise of irrationality and decline of reason may also be part of a wider global trend."

In Kapitza's statement that follows we find this paragraph:

> "Pseudoscience is even observable in high levels of the academic establishment. A well-known mathematician is publicizing for a new chronology of world history where there is no place for the Middle Ages and a thousand years of history are thrown out. . . these works are published and discussed in mass media. *Work on cold fusion and other marginal effects are supported and publicized, for the level of expertise and often the great persuasive power of these pseudoscientists leads to the support of their ideas. [Editor's Note:* Our *Italics.]* Where then are the limits of public debate and of professional honesty? Or is this all a transient phenomenon? Out of chaos will a new order finally come? These are not easy issues to resolve. Time and again the public is persuaded, if not fooled, on important matters of professional interest, often amplified by the media."

Kapitza is listed on the roster of CSICOP Fellows and for ten years he was "in charge of the Russian edition of *Scientific American*."

So, Kapitza says that cold fusion is "pseudoscience" and he alludes to "limits of public debate." Does he, like his CSICOP brethren outside Russia, want "pseudoscience" like cold fusion expunged from public discussion? Apparently. I would ask Kapitza: Where are *your* limits of "professional honesty"? Do you have any? Have you read the cold fusion literature published by your scientific colleagues in Russia, or is it just your habit to fling bigoted words toward experimental results that you have not examined? Have you attended international cold fusion conferences? Have you attended the cold fusion conferences in Belarus? We know you haven't, so why not just hold your peace on this matter—refrain from claiming to be knowledgeable on the important scientific issue of cold fusion.

Lack of intellectual curiosity (or is it lack of integrity?) is common among the leadership of CSICOP in the matter of cold fusion. On the morning of July 14, 1998, I called *Skeptical Inquirer's* editor, Kendrick Frazier, to ask him, among other things, what research or literature search he had done on cold fusion. He rebuffed me, saying that he was too busy to talk, because he was on deadline on an editorial project. We spoke briefly; he was transparently irritated. He said, "I know who you are." He said that he did not want to talk with me because, "We would have diametrically opposed views." I said, "Oh, what research have you done to come to your conclusions about cold fusion." I had thought that the careful investigation of "diametrically opposed views" was part of the work of CSICOP. Perhaps I was mistaken. Frazier said, "I am not an investigator, I am an editor." The conversation ended with Frazier stating that he had nothing further to say. I would have thought that CSICOP's journal editor should have been delighted to have a conversation with the "other side." He could then cite his exchange as part of CSICOP's "research by objective and impartial inquiry in areas where it is needed."

James Randi.
Photo: ©Enrico Ferorelli.

I note that we have extended to CSICOP a continuing complimentary subscription to *Infinite Energy*—as we often do to other influential national media. CSICOP's library has been receiving *IE* almost since our magazine's inception. Now in 1998 I have extended to editor Frazier himself a complimentary subscription—directly to his New Mexico address. Prior to my phone conversation with Frazier, I had told the person who answered the phone that Frazier should contact cold fusion scientists in New Mexico—Dr. Edmund Storms, formerly of Los Alamos National Laboratory (LANL); Dr. Tom Claytor, still at LANL; and Dr. Dennis Cravens. She took down their names. None of these people were contacted by Frazier—not that I am surprised. This is part of the intellectual bankruptcy of CSICOP, its Fellows, and its science journalist editor.

In *SI's* July/August 1998 issue (Vol. 22, No. 4), Frazier highlights two items from Robert Park's American Physical Society "What's New" electronic newsletter. The banner headline of the piece (signed by Frazier) is "APS Worries about Nonsense in Name of Science—Cold Fusion Transforms Again." Park's April 17 barb against cold fusion follows his note about the "alarming spread of nonsense calling itself science." Park: "Cold Fusion-7: Vancouver meeting reports more than heat—The preferred term among believers is now 'chemically assisted nuclear reactions.' Nowadays it seems to transmute gold into base metals, neutralizes radioactivity, and works fine with ordinary water. We note that the probability of N miracles is equal to the probability of one miracle to the Nth power." If *SI* readers imagine they are getting the straight story about cold fusion from thoughtless assaults such as Park's, they are mistaken.

To review some of the attacks on cold fusion by other high-profile CSICOPers:

From magician James Randi, a long-time CSICOP member and founder: "The 'cold fusion' farce should have been tossed into the trash heap long ago, but justifiable fear of legal actions by offended supporters has stifled opponents. . . cold fusion is a dead duck, the earth is not flat, and the fault lies not in our stars, but in ourselves." (From *American Physical Society News*, June 1994). Leap to *SI* July/August 1998 (Vol. 22, No. 4) and we find a report on Randi's latest "Pigasus Awards," one to ABC-TV News "for their unquestioning and enthusiastic endorsement of 'cold fusion,' ESP. . .[etc.] and all sorts of junk science. . . [blah, blah, blah]." We profiled Randi's nonsense in "Words to Eat" (*IE* No. 20).

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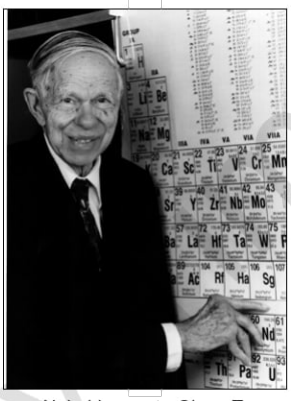
Nobel Laureate Leon Lederman, another Fellow of CSICOP: "If those university of Utah chemists who thought they had discovered cold fusion had understood Faraday's law of electrolysis better, perhaps they would never have embarrassed themselves as well as the rest of us." (In *The God Particle*, by Lederman and Teresi, 1993), See *Words to Eat IE* No. 21.

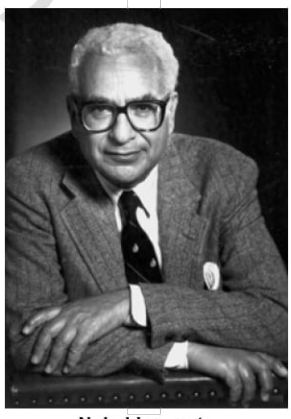
Physics Nobel Laureate Leon Lederman. Photo: Courtesy, Fermilab Visual Media Services.

CSICOP Fellow, Nobel Laureate Glenn Seaborg played a major role in getting the U.S. DoE and the scientific community at large off on the wrong foot in 1989. By his own admission, on April 14, 1989 (Only three weeks after the Utah announcement!), Seaborg told President Bush in the Oval Office of the White House, ". . . it is not due to nuclear fusion, but on the other hand it must be investigated." Thus was launched the sham investigation by the DoE, leading to its rush-to-judgement negative report only several months later. CSICOP editor Frazier is apparently so enthralled by his Nobel Laureate Fellows that he had no problem with this Seaborg statement—on its face a bald-faced admission of "rejecting claims on *a priori* grounds, antecedent to inquiry," contravening the supposed principles of CSICOP. These words appeared in *Skeptical Inquirer's* November/December 1997 (Vol. 21, No. 6) interview profile of Seaborg, "The Elemental Man." (See *IE* No. 15/16, p. 3, pp. 66-67).



Nobel Laureate Glenn T. Seaborg. Photo: Courtesy Lawrence Berkeley Laboratory

Another CSICOP Fellow, John Maddox, editor emeritus of *Nature* magazine, played a major role in unleashing anti-scientific bigotry against cold fusion. He wrote slanderous editorials. Violating all the canons of scientific ethics, while Maddox was editor *Nature* refused to publish scientific correspondence from several Ph.D. electrochemists that criticized precise mathematical and procedural points in the allegedly null-results in Caltech's cold fusion calorimetry, which had been published in *Nature*. (The devastating correspondence was subsequently published in several other peer-reviewed journals, thus circumventing *Nature's* unethical suppression of scientific discourse about its own publication.) This was not surprising with Maddox at the helm. Early on Maddox had written: "It seems the time has come to dismiss cold fusion as an illusion of the past four months or so." (July 6, 1989, *Nature*). "I think that, broadly speaking, it's dead, and will remain dead for a long, long time." (1991, in the NOVA television program "Confusion in a Jar." See *IE* No. 18, "Words to Eat.")



Nobel Laureate Murray Gell-Mann Photo: Courtesy Karsh/Caltech

CSICOP Fellow, Nobel Laureate Murray Gell-Mann, had this to say at a public forum (lecture at Portland State University, as reported in *IE* No. 19, p. 47). in 1998: "It's a bunch of baloney. Cold fusion is theoretically impossible, and there are no experimental findings that indicate it exists."

Though CSICOP's Fellows are among the leading people who trash cold fusion research, it is interesting that cold fusion has not so regularly come under fire in the *Skeptical Inquirer's* pages—certainly not in the manner that other heresies come in for ritual brutality. Perhaps there is a conscious or subconscious awareness that CSICOP figureheads are in no position to adequately judge cold fusion? But I fear that gives them too much credit! CSICOP members could not possibly be unaware that many scientists continue to perform cold fusion research, participate in international meetings, and even engage in commercial activities involving cold fusion energy and low-energy transmutation. The glib CSICOP explanation for these continued studies and activities is, of course, "pathological science."

Still, whenever there is a tid-bit of ironic cold fusion bashing to purvey, *Skeptical Inquirer* obliges. A sampling:

In *SI* May/June 1998 (Vol. 22, No. 3) Kendrick Frazier, ignorant of the quality and magnitude of the scientific effort in cold fusion physics, and unwilling even to contact cold fusion scientists who live near him, makes hay over the University of Utah's abandonment of its cold fusion patent application. He does not tell his audience, of course, of the reason for this Utah action: the U.S. Patent Office's killing of the Pons-Fleischmann patent on spurious grounds—a matter that we have discussed extensively in *Infinite Energy* (see in particular *IE* No. 11). Frazier's signed *News and Comment* is bannered, "Cold Fusion Saga Ends at Its University of Utah Birthplace." Frazier is in no position to discuss the *end* of anything in cold fusion, but of course his propaganda no doubt delights CSICOP "true believer" skeptics. He labels cold fusion scientists "remaining die-hard devotees." He cites the 1989 DoE report like a mantra and says, "With only a few exceptions, the results [of initial cold fusion attempts at confirmation] were negative. The claims did not stand up."

In *SI* January/February 1998 (Vol. 22, No. 1), we have science writer Robert Scheaffer, whose "beat" is usually UFO debunking, mocking one of the world's greatest electrochemists: "Another advance in science receiving belated recognition involves Professor John Bockris of Texas A&M University, who was recently awarded the

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celebrated Ig Nobel Prize for Physics by the *Annals of Improbable Research* at Harvard. Bockris is a leading researcher in the field of cold fusion, whose accomplishments have been prominently featured in *Infinite Energy* magazine. However, the prize was actually awarded for his experiments demonstrating the chemical transmutation of base metals to gold. Bockris did not travel to Cambridge to pick up his prize."

In *SI* May/June 1997 (Vol. 21, No. 3), New Mexico colleague of *SI* editor Frazier, John Geohegan, president of the CSICOP affiliate, New Mexicans for Science and Reason, praises the John Stossel program on ABC Television ("Junk Science," January 9, 1997). He begins with, "What a pleasure it is to see responsible treatment of science and pseudoscience on network television." He praised Stossel's treatment of cold fusion: "Next came an analysis of incentives for scientists to jump to the wrong conclusions: greed; powerful empires wishing to control science; fame and glory, as in the case of cold fusion. . ." Gee, "powerful empires wishing to control science"? For a moment I thought Schaeffer was talking about the hot fusion program, whose members at MIT and Princeton played such key roles in assaulting cold fusion! Stossel's journalistic travesty, by the way, was dissected in *Infinite Energy*, No. 20.)

> *Unfortunately, as these true believer skeptics are being corrected, their mouths and fingers operate open-loop, powered by brains apparently devoid of scientific curiosity.*

Ironically, Kendrick Frazier began *Skeptical Inquirer's* coverage of cold fusion in the Summer 1989 (Vol. 13, No. 4) issue with a relatively balanced appraisal, in part: "The case, it seemed, could turn out to be an epochal scientific achievement of overarching importance, or it could turn out to be an artifact, a mistake, or a delusion. Or perhaps it could be something in between, some previously unknown reaction that is new and important, but less significant scientifically and practically than originally claimed." Frazier signed off that article on May 2, 1989—the day after MIT Plasma Fusion Center's planted a story in the *Boston Herald* alleging that cold fusion was "scientific shlock" and "maybe fraud."

Frazier was even then beginning to buy onto the emerging propaganda line against cold fusion by MIT hot fusioneers and their Caltech colleagues: "By early May, with several major research institutions reporting negative results in their own experiments and serious flaws in the Utah ones, the balance seemed to tip strongly against the Utah results." It was downhill from there with Frazier at the wheel.

Editor Frazier had not a word about cold fusion in the next issue Fall 1989 (Vol. 14, No. 1)—nothing, in fact until the Winter 1990 issue (Vol. 14, No. 2), therefore a significant gap in coverage in the crucial first phase of the cold fusion controversy. In this Winter 1990 issue we find Milton Rothman's "Cold Fusion: A Case History in 'Wishful Science'?" He concluded: ". . . overenthusiasm and apparent greed and hubris changed a minor event into a major embarrassment for all of science. The manner in which scientists are perceived by the public has been diminished by this affair."

In the Fall 1990 issue (Vol. 15, No.1), Frazier noted that Gary Taubes had an article in *Science* magazine (June 15, 1990) in which, Frazier said: " [It]openly discusses for the first time questions that have been raised about the possibility that the tritium [in Texas A&M University cold fusion experiments] may have resulted from, if not inadvertent contamination, 'something more insidious.' Reports that researchers both at A&M and elsewhere have asked that questions about 'possible fraud' be resolved." Quoting Taubes approvingly, he wrote that the episode. . . "has become a case study in the damage done when questions of fraud, legitimately raised, are not seriously addressed by either the lab chief or his institution." Just to be sure everyone got the point, Frazier cited the Taubes article *again* in the Winter 1991 issue (Vol. 15, No. 2), and for good measure cited the negative conclusions of the U.S. DoE cold fusion final report.

Frazier, in all the tawdry pages of CSICOP's subsequent cold fusion coverage has never told his readers that the "fraud" allegation by character-assassinating journalist Taubes was officially found to be false. It was disproved by real scientists. Frazier has never informed his readers that there were subsequent publications by others in peer-reviewed journals, compellingly proving the production of tritium in cold fusion experiments. Yes, nothing excites CSICOP people more than the word "fraud" for claimed phenomena that don't fit their paradigms.

By the spring of 1991 (Vol. 15, No. 3), Frazier had "gone all the way." He quotes approvingly an article by journalist Bill Broad of the *New York Times* (October 30, 1990), "Cold fusion Still Escapes Usual Checks in Science": "Excellent report on the cold fusion debacle, focusing on why the conventional checking mechanisms of science apparently failed to resolve the issue decisively enough and how the process of science 'can be subverted by dedicated mavericks who defy the canon's of science.'"

This is by no means an exhaustive listing of all the ignorant insults flung at cold fusion research in the pages of *Skeptical Inquirer*, but I should mention that my book *Fire from Ice: Searching for the Truth Behind the Cold Fusion Furor* (Wiley, 1991) was, as expected,

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unfavorably reviewed. NASA physicist Steven N. Shore published his review in the *SI* spring 1992 issue. (Frank Close's *Too Hot to Handle*, 1991 had been favorably reviewed two issues earlier.) Shore's review appeared under the line: "Seeking 'Resurrection' for Cold Fusion." He concluded: "If you want to see the other side of this issue, and to have a very clear look at the way that science can be distorted in its presentation, then Mallove's book is essential reading."

If it is an honor to be attacked by science bigots such as Shore who never took the time to study the archival papers or attend scientific meetings on cold fusion, I am honored. On the other hand, I prefer what Nobel Laureate Julian Schwinger (who was maligned for his cold fusion theorizing) had to say about *Fire from Ice* on the book's jacket: "Eugene Mallove has produced a sorely needed, accessible overview of the cold fusion muddle. By sweeping away stubbornly held preconceptions, he bares the truth implicit in a provocative variety of experiments."

In due course, these hypocrites at CSICOP and their allies will receive long-overdue recognition as betrayers of the truth. Science will correct them.

Frazier promoted John Huizenga's book *Cold Fusion: The Scientific Fiasco of the Century* (University of Rochester Press, 1992), quoting favorably Huizenga's bottom line: "The cold fusion fiasco illustrates once again, as N-rays and polywater have done earlier, that the scientific process works by exposing and correcting its own errors."

Yes, indeed, Frazier *et al.* are being corrected even as we speak, in laboratories worldwide. Unfortunately, as these true believer skeptics are being corrected, their mouths and fingers operate open-loop, powered by brains apparently devoid of scientific curiosity. In the matter of cold fusion, the twitching corpse of skepticism run amok—the egregious malfeasance of *Skeptical Inquirer* and CSICOP— is all too evident.

Kendrick Frazier's subsequent and continuing intellectual fraud in *Skeptical Inquirer's* coverage of cold fusion is commensurate with the smear of outstanding scientists such as Prof. John Bockris by journalist Gary Taubes. For some time we expect it will continue to be "case closed" on cold fusion for the pontificating, fact-avoiding science cops at CSICOP. In the pages of *SI* more knives will no doubt come out, wielded by the professional debunkers of cold fusion. These are the pathological skeptics, who solemnly intone about "pathological science," "wishful thinking," and "fraud." In due course, these hypocrites at CSICOP and their allies will receive long-overdue recognition as betrayers of the truth. Science will correct them.

Jed Rothwell's Letter to a CSICOP Affiliate Publication

To: The Editors December 23, 1998
Skeptic Magazine, P.O. Box 338, Altadena, CA 91001

Dear Sirs:

Your latest issue (Vol. 6, No. 3) included articles criticizing cold fusion by Park (p. 16) and Gorman (p. 65). Both claimed that the effect was not reproduced. This is incorrect. Excess heat and tritium from cold fusion have been widely reproduced at many world-class laboratories, including several U.S. National Laboratories, the French Atomic Energy Commission, Mitsubishi and so on. Many scientific papers describing this work have been published in peer-reviewed journals. Park and Gorman may claim that all of these papers are incorrect, but they cannot claim the papers do not exist.

If Park and Gorman believe that all of these papers are incorrect, they should base their arguments on a detailed examination of the experimental data. Blanket statements that cold fusion is "junk science" have no meaning, and admit no response by the authors. How can anyone respond to such vague statements? If Gorman thinks this research is not up to snuff, he should list specific reasons why. He might explain, for example, why helium is confused with deuterium at the University of Texas and Rockwell International, or why the three methods for detecting tritium employed at Los Alamos failed simultaneously in experiment after experiment over the last eight years. I have read hundreds of papers about cold fusion. I have a bibliography listing more than 1,200 papers. As far as I know, neither Park nor Gorman has published a paper or technical critique, so I doubt they have any basis for their claims.

Cold fusion is an experimental observation, not a theory. It is valid because it has been widely replicated at high signal to noise ratios. You cannot disprove experimental evidence by pointing to a theory or an equation. Instead, you must slog though hundreds of papers and find specific errors or artifacts in every one of them. Some of these papers are strongly positive —irrefutably positive, in my opinion. Some are negative. Many are muddled or inconclusive, because cold fusion is difficult to replicate. The best laboratories, like Mitsubishi, reported that seven out of seven experiments over the past two years produced excess heat and gamma rays. In other successful laboratories about half of the cathodes produce excess heat and nuclear ash. However, most labs only see heat in perhaps one in ten experiments, mainly because of problems with cathode materials: the metal does not absorb deuterium, or it absorbs but expands, warps and fractures. This kind of difficulty replicating an effect is not unusual in solid state and catalysis applications. In the 1950s, many transistor production runs failed, or produced only one or two working devices per hundred. That is why transistors remained more expensive than vacuum tubes for several years.

I encourage you to acquaint yourselves with some of the technical literature in this field. I presume you do not have time

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Transcript of BBC Radio 4 Programme "Today" October 20, 1998

Michael McKubre and Martin Fleischmann

We are encouraged that in the U.K. such excellent, open-minded reporting about cold fusion continues. Our appreciation to BBC radio reporter Mike Williams.—EFM.

PRESENTER: Do you remember Cold Fusion, the technology which promised limitless pollution-free energy to a world hungry for power? And do you remember the disappointment when researchers dismissed it as a false hope? Well, one of the men who helped its development, or claimed to, said that there was a conspiracy to discredit his work while laboratories across the world contain scientists continuing for the search for the technological Holy Grail. Michael Williams reports. . .

WILLIAMS: It's almost ten years since two professors at the University of Utah made an announcement which promised to change the world. Stanley Pons and his colleague, Martin Fleischmann, said they had harnessed the power of the sun—nuclear fusion—using a small piece of equipment which operated near room temperature.

FLEISCHMANN: What is on offer here is a source of energy which is accessible to everybody all over the planet. We could create any sort of society anywhere we want.

WILLIAMS: Fleischmann and Pons believed that within their electrochemical cell, minute chunks of matter at the heart of atoms, the nuclei, were merging, liberating energy in the process. Theirs was a simple but remarkable claim that more energy came out of the system then went into it. But within six months, the work had been widely dismissed by many researchers around the world who tried and failed to reproduce the results. Perhaps the most definitive study came from the U.K. Atomic Energy Authority at Harwell, where Nick Hans is the spokesman.

HANS: I know that the scientists concerned desperately wanted cold fusion to exist. They looked very hard, but they weren't going to be fooled. They investigated it very deeply with all the skills of nuclear scientists and electrochemists and, in the end of the day, they could not ascribe a cold fusion process.

WILLIAMS: But the data from Harwell was analyzed by others. Among them Michael Melich, who works for the United States military. He's a Professors of Physics at the U.S. Naval Post-Graduate School in California. He never gives interviews, but he did agree to issue a statement, a summary of his findings. . In the data from Harwell, he says, he found evidence of cold fusion. . .

STATEMENT: Our re-analysis strongly suggests that they had at least ten anomalous heat generation events. My own view of cold fusion is that the system has properties which are not understood. Not least of which is the ability to produce relatively large quantities of power.

WILLIAMS: Martin Fleischmann is drinking in the Fox and Hounds, a pub near the Wiltshire village to which he returned after his years in Utah. The water in his beer, he says, could be made to give up some of its atomic components, producing enough energy from one pint to run an electric fire for almost a year.

FLEISCHMANN: I have never been able to find the evidence that we were wrong, therefore, I continue, and other people continue too. I mean there are many people working on this in the world. There are clearly a number of industrial organizations who are interested and a certain number of government institutions.

WILLIAMS: There is a stigma associated with work in cold fusion. Distinguished scholars from around the world have dismissed the Pons-Fleischmann effect as mere experimental error. But there remains a small community of scientists who keep the faith. Among them Dr. Michael McKubre. . .

McKUBRE: We have done enough work to know that there is heat being produced which is well beyond any amount of heat that you can account for by chemistry, by known chemistry. The most likely source of that heat, in my view, is a nuclear process.

WILLIAMS: He's an electrochemist with SRI International which grew out of Stanford University in California and, in his lab alone, they've spent seven million dollars investigating the Pons-Fleischmann effect. Other more secretive laboratories are investigating too.

McKUBRE: We have an informal collaboration of four or five major research institutions in the United States, for example. The sponsors of these institutions may not want their involvement to be known.

WILLIAMS: Is the American Government involved in research to your knowledge?

McKUBRE: Yes it is, to my knowledge.

WILLIAMS: Can you tell me the nature of the involvement?

McKUBRE: I'd rather not. I can assure you that it's real. I know directly governmental sources are using U.S. governmental funds directly and specifically to do this research but some people, influential people in Washington, just don't want that to be the case.

FLEISCHMANN: It looks to me as though there has been a concerted attempt to try to stop this project going further.

WILLIAMS: Why?

FLEISCHMANN: You put me in a difficult position. I mean I can only answer this from my own experience. It had been my intention to explore the possible applications of this in military technology and maybe people are worried about that particular aspect. My own thoughts on this will probably—should probably—accompany me to the grave.

PRESENTER: That report was by Mike Williams. Fascinating stuff.

Rothwell continued from page 45

6. S. Manes, "Windows 98: $90 Worth of Duct Tape," *New York Times*, June 25, 1998.
7. Max Planck, *A Scientific Autobiography*, 1949, p. 33 (Translated by E. Gaynor).
8. F. Dyson, *Infinite in All Directions* (Harper and Row, 1988), p. 148.
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ROCKET WOMAN – AN INTERVIEW WITH ANGIE EDWARDS

by Susan Seddon

Life may be too short to stuff mushrooms for most high-powered, female executives these days, but Angie Edwards is one Superwoman who really does manage to do it all.

In addition to being an extremely busy housewife, and Mum to threedelightful young ladies, she is the Director and Company Secretary of Rocket Publishing Company Limited—the U.K. arm of the vast Arthur C. Clarke literary empire.

Angie, Sir Arthur's niece, runs the company from what she cheerfully describes as "a hell-hole near the bathroom chez nous" in the lovely home she shares with Welsh-born husband, Stephen; their daughters Georgina (12), Harriet (9), Lydia (6) and a very friendly golden labrador named Buster.

Before they settled in East Yorkshire, Stephen's career as a Chartered Surveyor necessitated some interesting house moves. They travelled in New Zealand for a year; lived in the English Lake District and even, for awhile, occupied a cottage on the historic Chatsworth Estate in Derbyshire, ancestral home of the Dukes of Devonshire. Renowned chemist/physicist Henry Cavendish was a grandson of the 2nd Duke.

Angie joined the Board at Rocket in 1991. She now directly administers many of her famous uncle's U.K. business and charitable interests, including the Arthur C. Clarke Literary Award for Science Fiction.

This prestigious event is hosted annually by the London Science Museum and the considerable prize of £1,000 attracts a great deal of competitive interest among science fiction writers.

She also administers requests for financial assistance, in the form of grants, to U.K. students aspiring to a place at the International Space University in Strasbourg, an organization enthusiastically supported by Sir Arthur, who once served as its Vice-Chancellor.

Angie good-naturedly shrugs off all accusations of dynamism, but it's quite plain that her busy lifestyle would leave many others flagging.

She maintains very close contact with her uncle via telephone and email and is regretful that she hasn't yet been able to arrange to visit him in Sri Lanka.

Not all of the phone calls between them, however, are confined to business. Arthur, she says, has a wicked sense of humor and will often call her simply to relay a stream of jokes he discovered on the Internet, or occasionally, "he'll just phone for a friendly chat."

"I don't think of him at all as a world-famous author," she said. "Just as a wonderful uncle."

PERPETUAL MOTION: AN ANCIENT MYSTERY SOLVED?

Comments by John Collins

Following the generous coverage given to reviews and articles about my book *Perpetual Motion: An Ancient Mystery Solved?*, I would like to respond to some criticisms by Dr. Thomas Phipps Jr., and the ideas put forward by Steve O'Donnell, who attempted to explain Bessler's wheel through an ingenious arrangement of magnets fixed to a hidden drive wheel.

Promoting a controversial theory is a hazardous excercise at the best of times, but when an amateur such as I seeks to correct the elite in the world of physics, by suggesting that their view of certain accepted processes might be incomplete in some basic way, he or she can expect a range of responses which stretch from complete indifference, through ridicule and scorn, to a thorough literary lambasting. So it was with some trepidation that I looked forward to Dr. Tom Phipps' review of my book. I believe that his main criticism was with the final chapter—and the whole theory which I had proposed!

I should explain how the final chapter came to be written. The book had been finished (minus its present final chapter) and had been the subject of several readings by various people. The general opinion was that there needed to be something which explained what benefits such a machine as Bessler's could provide in the 21st century, otherwise what was the point of the research. I was encouraged to allow my imagination free reign and to try to anticipate the ways that such a device (if it were ever built) would develop and what effect it would have on the world in general. That I succeeded is clear to me because despite Dr. Phipps' reservations, others have stated that it is their favorite chapter. Admittedly these people shared similar views to those whom Dr. Phipps calls "children with flowers in their hair," but the chapter would appear to be balanced and was a necessary corollary to the book. Dr. Phipps humorously dismissed my imaginings with a succession of mixed metaphors but in fact all of my suggestions were, I believe serious and realistic. It may be difficult to imagine worldwide usage of a permo machine such as I suggest, obtaining its power directly from gravity, but in fact there is an excellent explanation of such a phenomenon available, and if proven true, the results would be undoubtedly as I described them in my book. Certainly at this time of approaching global recession, a device such as Bessler's wheel, with its simple technology and many applications, could provide necessary work for millions of people who might otherwise be floundering in the mud of unemployment and redundancy.

Dr. Phipps felt that my stated wish that nuclear power electricity generators should be closed and de-commissioned and proposals for new ones cancelled was naive because I suggested that Bessler's machine provided a safe, clean, cheap alternative, and yet no proof existed that my theory was tenable. I understand his viewpoint and certainly one might assume from reading the text in my book that I thought that this could be accomplished immmediately, leaving the way clear for Bessler's technology. In fact, I am well aware of the way these things work. De-commissioning a nuclear facility is not undertaken lightly, speedily, or cheaply. What I wished to convey was the importance, in my opinion, of conducting research into the means of duplicating Bessler's wheel, always assuming that you accept my arguments for its validity. Running in tandem with such research was the earnest wish that at least feasibility

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studies should be carried out on the ways such de-commissionings could be instituted with speed once a breakthrough had been achieved in the research. I don't know precisely how long it takes to close down and make safe a nuclear plant but I should imagine something in the order of ten years or more; and that only after thorough planning and research has been carried out at each unit to devise the most effective means. This will leave time to discover, yea or nay, if Bessler's machine is of practical value.

I was pleased with Dr. Phipps' enthusiasm for my writing and disappointed that in the end he felt that my flotilla of facts, marshalled in support of my contention that gravity could provide a source of energy for the future, had run aground on the sands of a conservative force. You might think that I would have thought of that and therefore rejected Bessler's machine as valid, but the reviewer seems to infer that I hadn't. If that is so then the fault lies with my writing. I did indeed concern myself with this very problem for years before coming to what I believe is the solution to the problem. The reason I spent so long debating the problem with myself is because I had come to the conclusion that Bessler's machine was genuine. The reasons for this are provided in detail in my book, and although Dr. Phipps has come to a decision regarding this evidence, which differs from my own, it is because of his education and knowledge of the world of Physics which informs him of the impossibility of deriving power, indefinitely, from gravity. I, on the other hand, with a similar amount of experience in the world of mechanics, cannot reject that evidence, because I know it to be impossible to fake and must therefore seek another solution. The solution that I have found takes the form of a loophole, requiring no over-turning of physical laws, merely a refinement of existing beliefs.

The principal of a conservative force is simple but can be misleading. I have always understood that energy consisted of potential energy (PE) and kinetic energy (KE). PE is stored energy and KE is energy in motion. A conservative force is defined as PE and cannot be KE. Bessler's wheels required a slight push along until the first weight was heard to fall at which point they began to gather speed until they had accelerated to their maximum. We can infer from this that it was the action of the weights, rather than their position within the structure, that was more important. In support of this there is the strong evidence from many reliable witnesses who described the loud noise which accompanied the rotating wheel. It seems to me that they must therefore have been impacting on the falling side of the wheel, thus transferring their energy to the wheel. If this is true then the concept of a conservative force is not appropriate here, because what we are seeing is KE in action. The term conservative force does not apply to Bessler's wheel except when it was stationary, and during the fall of the weight. Once that weight struck the side of the wheel, PE no longer applied and KE took over. Again, if an alteration was made to the position of the weight and its distance from the center of gravity, for the upwards part of the revolution, then the figures which applied for the downwards motion are different to those which applied for the upwards part and the restrictions of a conservative force no longer applied. Another way to look at it is to imagine a weight falling on one side of a wheel. To raise that weight on the upward side would require additional force to lift it back to its starting point. This is because gravity is a conservative force, and work is measured by the displacement of the weight during the movement. This displacement is measured vertically, but the object which fell could take any path. Upwards or downwards, it doesn't matter, if the force which allowed it to fall is the same as the force required to raise it, then it will not rise to the starting point because it will have expended some energy during its fall, and the rest will have been dissipated turning it round to send it back up. But what everyone—and I do mean everyone—has forgotten or not noticed is that if you wish, as perpetual motionists do, to have the weights move closer to the center of gravity on the rising side of the wheel, then you have to provide a force to move them inwards or outwards, otherwise you cannot achieve that operation. Now if you add a force to move the weights accordingly, you have altered the equation which applied to the weights and the wheel when the weights began to fall. The turning momentum which applied as the weights fell has been increased for the lifting part of the cycle, so you cannot apply the principle of a conservative force. And, of course, the additional force which would be required to move the weights both inwards and outwards can still be gravity.

I had better reiterate my professed belief that the claim that perpetual motion is impossible in any practical sense may not necessarily be true. It depends on whose definition you are using. To get more energy out than is put in makes a machine more than 100 percent efficient, and to me is clearly absurd. However, if a machine is being driven by falling weights, then hypothetically it is receiving an input of energy from the field of gravity every time a weight falls, and does not therefore conform to the legal definition of a perpetual motion machine, which is denied this avenue of supply.

There is one puzzling comment by Dr. Phipps. Towards the end he says, ". . . if Orffyreus knew a way to extract more energy from a gravity field than he put into it, he knew a trick I'll never know." Did Orffyreus put energy into a gravity field? I didn't know that. The energy is already there, waiting to be tapped, surely. But Orffyreus found a way to extract it, that's true.

Now Steve O'Donnell has worked hard to discover a way to show that Bessler (aka Orffyreus) was a fraud. His article which recounted his search for the truth shows that he is a resourceful man and possessed of some ingenuity, but not I believe, the solution. It is difficult to know where to start. He has assumed that magnets lay at the heart of the supposed deception, and why not? It would provide a simple solution to an awkward problem—how to explain the inventor's success otherwise. His ingenius contrivance is convincing—until you consider the facts. Let us assume that Steve's solution is the correct one. We still have the problem of explaining the following:

1) How was the wheel driven for 54 days, day and night without stopping?

2) Were the magnets so powerful that the load on the wheel, consisting of its weight at 700

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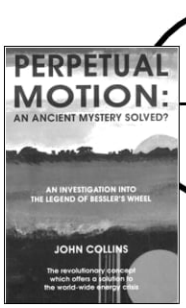

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TRANSMUTATION OF NUCLEAR WASTE PRODUCTS USING GIANT DIPOLE RESONANT GAMMA RAYS

Paul Brown, Ph.D.

Nuclear Solutions LLC.

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Delivered at the Cold Fusion/New Energy Symposium, October 11, 1998, Manchester, New Hampshire.

Editor's Note: Dr. Paul Brown's work on a novel form of nuclear waste remediation that employs gamma rays was the subject of an *Infinite Energy* feature story in the previous issue (*IE* No. 21). This proposed and patent-applied-for technology does not rely on new principles of physics, rather ones that were apparently simply overlooked in their applicability to nuclear waste treatment. In his talk at our Cold Fusion and New Energy Symposium on October 11, 1998, Dr. Brown continued to develop his ideas and presented some emerging experimental support for his process. —*EFM*.

Introduction

The single most important challenge facing the nuclear field (commercial and defense) is what to do with the nuclear waste. The Nuclear Waste Policy Act of 1982 commits the United States to geologic isolation as the best long-term solution for the final disposition of waste. However, after all these years and billions of dollars, there remain numerous questions concerning our ability to develop full reliance on a geologic solution to the waste management problem. Concepts such as the separation and transmutation of nuclear wastes that either eliminate or reduce their radioactive inventories are recognized alternatives. These alternatives have been studied for many years with the primary focus upon neutron absorption produced by a nuclear reactor or particle accelerator. However, these technologies are limited due to the high costs involved as well as the application of yet to be developed technology. Many believe that nuclear energy is not an acceptable option until its waste products can be disposed of in a demonstrably acceptable manner.[1]

Because there are potentially useful amounts of plutonium, uranium and rare metals in light-water reactor spent fuel, there is justification in considering light-water reactor spent fuel as an energy and materials resource rather than waste. Spent fuel reprocessing is applied in many countries to recover the reusable uranium fuel and reduce the volume of waste products.

A facility for reprocessing commercial reactor fuel operated at West Valley, New York, for a short period, and two large commercial reprocessing facilities had been constructed at Barnwell, South Carolina and Morris, Illinois, but not operated. The reprocessing of commercial reactor fuel was discontinued in the United States in the mid-70's. However, several nations, notably France, the United Kingdom, Russia, and Japan, continue to pursue reprocessing for limited recycle of plutonium into light-water reactors and for support of their ongoing breeder reactor development programs.

The high level radioactive wastes produced during reprocessing of spent nuclear fuels contain long-lived radionuclides such as the fission products Sr^{90} and Cs^{137} as well as numerous actinides. The management of high level waste is one of the most important problems in the nuclear industry. The dominant approach for high level waste disposal is geological storage and the studies for geological storage are proceeding in many countries. The transmutation of long-lived nuclides in high level waste to stable or short-lived nuclides by stimulating nuclear reactions is a desirable alternative approach for the reduction of high level waste. Several transmutation methods have been studied using neutron-induced reactions from fission reactors and D-T fusion reactor blankets, and proton reactions using proton accelerators.

Photonuclear reactions induced by gamma ray absorption by the nucleus, do not suffer the shortcomings of neutron reactions. Simply stated, the process is gamma irradiation with energies greater than the binding energy of the neutron to the nucleus. That is, a gamma photon of an energy equal to or greater than the binding energy which comes close to the nucleus is absorbed through giant dipole resonance resulting in the emission of a neutron.[2] This, well known, nuclear reaction has dramatic application to waste remediation as evidenced by Table 1.

Table 1.

H^3 ((γ,*n*) H^2 (stable)
C^{14} ((γ, *n*) C^{13} (stable)
Y^{90} (γ, *n*) Y^{89} (stable)
Ni^{63} (γ, *n*) Ni^{62} (stable)
Kr^{85} (γ,*n*) Kr^{84} (stable)
Co^{60} (γ, *n*) Co^{59} (stable)
Tl^{204} (γ, *n*) Tl^{203} (stable)
Sr^{90} (γ, 2*n*) Sr^{88} (stable)
Bi^{210} (γ,*n*) Bi^{209} (stable)
Tl^{204} (γ, *n*) Tl^{203} (stable)
Ba^{133} (γ, *n*) Ba^{132} (stable)
Pb^{210} (γ, 2*n*) Pb^{208} (stable)
Sr^{90} (γ, *n*) Sr^{89} (50 day half-life) - β^-[] → Y^{89} (stable)
Tc^{99} (γ, 3*n*) Tc^{96} (4.3 day half-life) - β^- → Ru^{96} (stable)
I^{129} (γ, *n*) I^{128} (25 minute half-life) - β^- → Xe^{128} (stable)
Cs^{137} (γ, *n*) Cs^{136} (13 day half-life) - β^- → Ba^{136} (stable)
Fe^{60} (γ, *n*) Fe^{59} (44.5 day half-life) - β^- → Co^{59} (stable)
Pb^{210} (γ,*n*) Pb^{209} (3.25 hour half-life) - β^- → Bi^{209} (stable)

There are about 300 different radioactive species generated by the operation of a nuclear reactor, primarily as a result of neutron capture and neutron-induced fission. The adverse impact of the various radionuclides varies because of the differences in the chemical behavior in the body of, and the radiations emitted by, the radionuclides. The risk focus of the radionuclides is related to waste disposal in a geologic repository. The most common release and exposure mechanisms from a repository involve ground water contacting the waste form followed by slow dissolution, transport of radionuclides to the accessible environment, distribution in the biosphere, and eventual uptake from food and water. Although hundreds of isotopes are present in spent fuel or wastes derived from them,

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only a few of them are important for disposal. These four isotopes Cs^{137}, Sr^{90}, I^{129} and Tc^{99} are the primary focus of concern for light-water reactor spent fuel, *i.e.*, nuclear waste due to their excess heat, groundwater solubility, or health risk.

The neutrons produced by the (γ, n) processing may in turn be used for neutron transmutation by the processes detailed in Table 2. For many fission products the neutron capture cross sections in a thermal spectrum can give substantial transmutation rates. The transmutation of Tc^{99} is characteristically much more effective in a thermal neutron spectrum, generally due to higher neutron capture cross section at lower energies.

Table 2.

$$Tc^{99} + n \rightarrow Tc^{100} \text{ (16 second) - } \beta^- \rightarrow Ru^{100} \text{ (stable)}$$
$$I^{129} + n \rightarrow I^{130} \text{ (12.4 hr) - } \beta^- \rightarrow Xe^{130} \text{ (stable)}$$
$$I^{127} + n \rightarrow I^{128} \text{ (25 min) - } \beta^- \rightarrow Xe^{128} \text{ (stable)}$$
$$Na^{22} + n \rightarrow Na^{23} \text{ (stable)}$$

The systematics of the giant dipole resonance, which characterizes the absorption of electromagnetic radiation by nuclei in the energy range from about 5 to 30 MeV, have been of interest since the discovery of the giant resonance itself. Over the years, the photoneutron cross sections for many nuclei have been measured with monoenergetic photons in numerous laboratories. All these data are presented in the Atomic Data Nuclear Data Tables (B. Berman and S. Dietrich). For most cases studied, the agreement is remarkably good.

The classical description of the dipole photon absorption process predicts that for spherical nuclei the total photon-absorption cross section is characterized by the Lorentz line shape,

$$\sigma(E\gamma) = \sigma_m / [1 + (E_\gamma^2 - E_m^2)^2 / E\gamma^2 \Gamma^2]$$

where σ_m is the peak cross section, E_m is the resonance energy, and Γ is the full width at half maximum. For deformed (spheroidal) nuclei, the collective picture predicts a splitting of the giant resonance into two components, corresponding to oscillations parallel and perpendicular to the nuclear axis of symmetry.

For medium and heavy nuclei, the Coulomb barrier inhibits the emission of charged particles at giant-resonance energies, and the photon-scattering cross section is always small above the (γ, *n*) threshold; therefore, the total photoneutron cross section is a good approximation to the total photon-absorption cross section.

The intrinsic quadrupole moment Q_o for a deformed nucleus can be computed from the expression,

$$Q_o = 2/5\, ZR^2\varepsilon = 2/5\, ZR^2 (\eta - 1)\, \eta^{-2/3}$$

Where the nuclear radius $R = R_o A^{1/3}$, Z and A are the atomic number and atomic weight, respectively, ε is the nuclear eccentricity, and the parameter η is the ratio of the major axis to the minor axis (for the prolate nucleus) given by the formula,

$$E_m(2)/E_m(1) = 0.911\eta + 0.089,$$

Where E_m (1) and E_m (2) are the lower and higher resonance energies of a two-component Lorentz-curve fit to the giant resonance.[3]

One of the important factors for the transmutations study is the transmutation rate. It is expressed as follows,

$$\lambda = N \int \sigma(E)\, \Phi(E)\, dE \approx N\, \sigma_{av} \int \Phi(E)\, dE$$

where N is the number of target nuclides, σ (E) is the excitation function of the relevant nuclear reaction, Φ(E) is the particle flux density, and σ_{av} is the average cross section. In order to estimate the transmutation rate, it is indispensible to know the accurate excitation function or average cross section.

Experimental Procedure

The source of gamma-radiation for these experiments used Compton-scattered gamma rays from the reaction Ni^{58} (n,γ) Ni^{59} as a source of γ-rays which presents an overall resolution of ≈ 3% in the range of 10 MeV. A sealed tube neutron generator is used, wherein ions are generated using a Penning ion source and accelerated through a potential difference to strike a titanium metal tritide target producing the D-T reaction

$$D + T \rightarrow n + He^4 \qquad En = 14.2 \text{ MeV}$$

Neutrons produced from the tube are emitted isotropically with a maximum yield of 10^{11} neutrons/second. The neutron flux delivered to the water cooled nickel converter is 10^9 neutrons/cm^2/second. The sample irradiation chamber was placed 30 cm downstream from the nickel converter and was water cooled constantly. The photon-energy resolution of the beam varies less than 300 keV at 10 MeV and 70 μamp.

Bremsstrahlung gamma photons may be produced by any other suitable manner such as an electron linear accelerator, such high-energy x-ray machines are commercially available. The tagged photon facility at the Nuclear Physics Laboratory of the University of Illinois produces monoenergetic tagged gammas by the bremsstrahlung monochromator method in the energy range of 11 to 16 MeV with an energy resolution of 80 to 120 keV and a beam current of the order of 10^6 photons per second. Continuous spectrum bremsstrahlung may be utilized, however, this is not as efficient nor does it have the tuneability of a monochromatic source.

All samples were mounted on a pure Ge detector having a relative efficiency of 31.2% and their γ-rays were counted for 4,000 to 160,000 seconds with a Canberra multichannel pulse height analyzer.

Results

Figure 1 shows an example of the γ-ray spectrum resulting from the (γ,n) transmutation of the Cs^{137} sample. Here we see three small but distinct photo-peaks of 340.6 keV, 818.5 keV and 1048.1 keV which are γ-rays from Cs^{136} nuclei which were produced in the target. The half-lives of these peaks agree with the reference value of 13.1 days to within 5%. This figure also shows a photo-peak of 667.7 keV γ-rays emitted from Cs^{132} nuclei reacted from the Cs^{133} (γ,n) reaction in the same way as Cs^{136} and decayed with the half-life of 6.47 days. The 661.7 keV γ-ray is due to the Cs^{137} still present. All other photo-peaks are γ-rays from natural radioisotopes such as K^{40} and Bi^{214}.

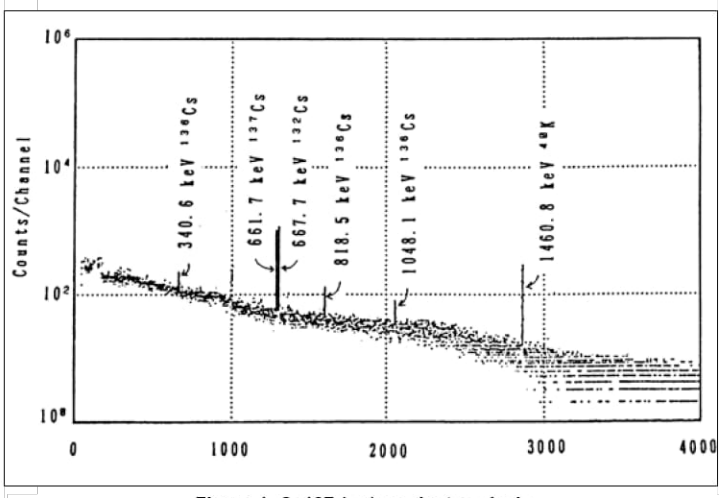

Figure 1. Cs137 (γ,n) product analysis

It is quite difficult to measure the very weak Sr^{89} and Cs^{136} activities produced from Sr^{90}(γ,n) and Cs^{137}(γ,n) reactions under

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ON CERTAIN CONDITIONS UNDER WHICH A PERPETUAL MOTION IS POSSIBLE

GEORGE BIDDELL AIRY, M.A.

Submitted to *IE* by R.A. Ford, Simplified Tech Service, Champaign, Illinois

George Biddell was a member of the Astronomical Society, Fellow of Trinity College, Cambridge Philosophical Society, and Plumian Professor of Astronomy and Experimental Philosophy at the University of Cambridge. Reprinted from the *Cambridge Philosophical Society Transactions*, Vol. 3, 1830. Paper read December 14, 1829.

It is well known that *perpetual motion* is not possible with any laws of force with which we are acquainted. The impossibility depends on the integrability *per se* of the expression $Xdx + Ydy + Zdz$: and as in all the forces of which we have an accurate knowledge this expression is a complete differential, it follows that perpetual motion is incompatible with those forces.

But it is here supposed that, the law of the force being given, the magnitude of the force acting at any instant depends on the position, at that instant, of the body on which it acts. If, however, the magnitude of the force should depend not on the position of the body at the instant of the force's action, but on its position at some time preceding that action, the theorem that we have stated would no longer be true. It might happen that every time that the body returned to the same position, its velocity would be less than at the preceding time. In this case the body's motion would ultimately be destroyed. On the contrary it might happen that the body's velocity in any position would be more rapid every time than at the time previous. In this case the velocity would go on perpetually increasing or the velocity might be made uniform if the machine were retarded by some constantly acting resistance; or, in other words, the machine might move with uniform velocity, and might at the same time *do work*: which is commonly understood to be the meaning of the term *perpetual motion*. If the machine had no work to do, the increasing friction, etc. would operate as an increasing work, and the velocity would be accelerated until the acceleration caused by the forces was equal to the retardation caused by the friction, after which it would remain unaltered.

If, however, the magnitude of the force should depend not on the position of the body at the instant of the force's action, but on its position at some time preceding that action, the theorem that we have stated would no longer be true.

For this idea I am indebted to the admirable account of the organs of voice given by Mr. Willis. The phenomenon to be explained was this: When two plates are inclined at an angle greater than a certain angle, it is found that the effect of a current of air passing between them is to give a tendency to open wider. When they are inclined at any angle smaller than that certain angle, the effect of the current is to make them collapse. If then the plates be supposed to vibrate through the position corresponding to that angle, the tendency of the forces at all times is to bring them to that position. Each plate is in the state of a vibrating pendulum and whatever be the law of force which acts on it, it is certain that if the force be the same when the plate is in the same position, this force will have no tendency to increase the velocity. The retardation arising from friction, etc. will therefore soon destroy the motion. But it is found, in fact, that the motion is not destroyed. What then is the accelerating force which keeps up the motion? Mr. Willis explains this by supposing that *time* is necessary for the air to assume the state and exert the force corresponding to any position of the plate, which is nearly the same as saying that the force depends on the position of the plate at some previous time. In this paper, which is intended to investigate the mathematical consequences of an assumed law, I shall not discuss the identity of these suppositions; I shall only remark that the general explanation appears to be correct, and that it clears up several points which always appeared to be in great obscurity.

Let us now consider the case of a vibrating body acted on by two forces, of which one is proportional to its actual distance from the point of rest, and the other proportional to its distance at some previous time. Putting $\phi(t)$ for the body's distance, the equation is

$$\frac{d^2 . \phi(t)}{dt^2} = -e.\phi(t) - g.\phi(t-d).$$

This equation I am unable to solve rigorously: but on the supposition that g is small, an approximate solution may be obtained from the formulae in the "Memoir on the Disturbances of Pendulums, etc." (*Cam. Trans.* Vol. 3, p. 109). Neglecting at first the small term, we have

$$\frac{d^2\phi(t)}{dt^2} = -e.\phi(t),$$

whence

$$\phi(t) = a.\sin(t\sqrt{e} + b).$$

Consequently

$$\phi(t-d) = a.\sin\left(t\sqrt{e} + b - c\sqrt{e}\right),$$

and therefore f in the formulae alluded to is

$$= ag.\sin\left(t\sqrt{e} + b - c\sqrt{e}\right)$$

The increase, therefore, of the arc of semi-vibration is

$$-\frac{1}{\sqrt{e}}\int_t^0 ag.\sin\left(t\sqrt{e} + b - c\sqrt{e}\right)\cos\left(t\sqrt{e} + b\right)$$

$$= -\frac{ag}{2\sqrt{e}}\int_t^0 \left\{\sin\left(2t\sqrt{e} + 2b - c\sqrt{e}\right) - \sin c\sqrt{e}\right\}.$$

To find the increase from one vibration to another we must take the integral between two values of t differing by $\frac{2\pi}{\sqrt{e}}$ and thus

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we obtain for the increase

$$\frac{\pi . \text{ag} \sin c\sqrt{e}}{e}.$$

I shall not occupy the time of the Society by a discussion of the different values of the increase corresponding to different values of c. I shall only remark that if $c\sqrt{e}$ be less than π, the arc of vibration increases continually. Nor shall I consider the cases in which c is supposed to be a function of the position or velocity of the vibrating body (which possibly might better represent the circumstances that originally suggested this investigation). My object is gained if I have called the attention of the Society to a law hitherto (I believe) unnoticed, but not unfruitful in practical applications.

CSCICOP continued from page 59

to review hundreds of papers, so I recommend you read a small selection of three representative papers:

M. McKubre *et al.*, "Isothermal Flow Calorimetric Investigations of the D/Pd and H/Pd Systems," *J. Electroanal. Chem.* 368 (1994) 55, enclosed. (You could read the full EPRI report instead, Development of Advanced Concepts for Nuclear Processes in Deuterated Metals, EPRI TR-104195, but it is 342 pages. I enclose the Summary and Abstract.)

M. Miles *et al.*, "Anomalous Effects in Deuterated Systems," NAWCWPNS TP 8302, September 1996, Naval Air Warfare Center Weapons Division, China Lake, CA 93555-6100, 96 pages. This was reprinted in our magazine. I would be happy to send you a copy.

T. N. Claytor, D. D. Jackson and D. G. Tuggle, "Tritium Production from a Low Voltage Deuterium Discharge on Palladium and Other Metals," Los Alamos National Laboratory. You can read this on the Internet: http://wwwnde.esa.lanl.gov/ cf/tritweb.htm

I would be happy to write a short article for your magazine about this subject.

Sincerely, Jed Rothwell

Editor's Note: Those open-minded CSCICOP followers who would like to learn more about the history of cold fusion science, from the perspective of those who have come to accept the experimental evidence for the phenomenon, are urged to examine the reports mentioned by Jed Rothwell. They might also wish to read:

- *Fire from Ice: Searching for the Truth Behind the Cold Fusion Furor* by Eugene F. Mallove, Sc.D., John Wiley & Sons, 1991 (available from *Infinite Energy* magazine)
- *Nuclear Transmutation: The Reality of Cold Fusion* by Dr. Tadahiko Mizuno, *Infinite Energy* Press, 1998, translated from the Japanese by Jed Rothwell, available from *Infinite Energy* magazine.
- Seminal references on cold fusion listed on the *Infinite Energy* magazine web site, http://www.infinite-energy.com

Nuclear Transmutation: The Reality of Cold Fusion

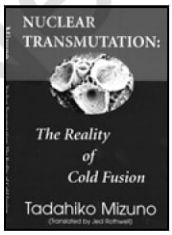

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Table 3

Nuclide	Reaction	G,N Threshold (MeV)	G,2N Threshold (MeV)	Max Energy (MeV)	Maximum Cross Section (mB)	Integrated Cross Section (mB)
Cs-133	G,N	9.0	16.2	15.31	321	1828
Cs-137	G,N	8.0	—	13.0	360	—
Sr-90	G,N	—	—	15.8	194	1311
I-127	G,N	9.1	16.2	14.9	252	1601

intense Sr^{90} and Cs^{137} target activities, respectively, and moreover there is no possible chemical separation method of Sr89 from Sr^{90} and Cs^{136} from Cs^{137}. The Cs^{136} atom decays to stable Ba^{136} with a half life of 13.1 days, which is a much shorter half-life than the 30.2 years of the Cs^{137} atom.

Mixed Waste

All work to date has been performed on chemically pure isotope samples. However, real world applications for nuclear waste reduction will require the treatment of mixed waste products to keep costs down. We have theoretically examined the treatment of mixed waste products and conclude that such treatment is possible. The reasoning for such a conclusion is fairly straight-forward.

The (γ,n) threshold for the radioactive isotopes is generally (see Table 3) between 6 and 9 MeV, while the (γ,n) threshold for the stable elements is generally between 15 and 30 MeV. This means that by using a gamma beam with a mean energy lower than 15 MeV would not result in (γ,n) activation of stable isotopes. Further, most of the isotopes present in mixed waste become stable or short lived through the (γ,n) reaction. A quick review of Table 1 shows that the (γ,n) process applies directly to the well known radioisotopes. Now, it is realized that some gamma activation does result, as it does with any high-energy x-ray source, but this activation is also short lived.

What remains to be done is the analysis of mixed waste samples, followed by prediction of the resultant products, and if acceptable, then actual testing of (γ,n) on such a mixed waste sample.

Conclusion

Application of the photo-remediation method would affect the design and long-term performance of nuclear repositories if not eliminate the need all together. This would provide for the selected recovery of fission products from spent fuel. The waste going to the repository would have less thermal power, would contain a reduced quantity of certain isotopes, occupy less volume, and could be incorporated in waste forms with good integrity. Reduction of the decay heat present in the waste product would also allow the repository's capacity to be increased, thereby eliminating or postponing the need for a second repository.

We have seen the application of the (γ,n) reaction to Cs^{137} yields the predicted results, and presented this data herein. The average cross sections of Cs^{137} (γ,n) is about 10 to 20% larger than that of Cs^{133} (γ,n) or about 360 mB; the threshold energy for (γ,n) is 8 MeV with a peak at 11 MeV and a maximum energy of 13 MeV. This data will be very useful in the evaluation of the transmutation studies using photonuclear reactions.

Application of this method to transmute nuclear waste products is a viable method for reducing the volume of nuclear waste. The solution of the nuclear waste problem also makes nuclear power a viable option.

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Technical Note Submitted to *Infinite Energy* as a Letter to the Editor:

AN EXPERIMENT INDICATES THE NUCLEAR FUSION OF THE PROTON AND ELECTRON INTO A NEUTRON

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Abstract

An experiment was performed that indicates that a proton and an electron may fuse at low temperatures and at low energies to form a neutron plus a neutrino. The aim of the experiment was to obtain the nuclear fusion of protons and electrons to neutrons at low energies.

The Experiment

As we shall see, the primary task of the experiments was to have the proton essentially at rest, and the electron having a minimal threshold energy of 0.80 MeV. An electrolytic cell was employed, having a Pt-anode and an Al-cathode with dimensions of 1 mm x 1 mm x 1 mm. (This was a fine aluminum gauze of cubical form suspended by a Pt electrical lead wire.)

The anode was made of pure Pt shaped as a gauze cylinder, having a skeleton made of 1 mm diameter wires supporting the spot-welded mesh made of 0.35 mm diameter wire. The mesh dimension was 5 x 4 mm^2 and the anode overall size was 48 mm high with a 12 mm inside diameter.

The cell itself was a cube of Pyrex glass having an edge dimension of 100 mm. It was open on its top. For the electrolyte, we used an alkali metal sulphate, Na_2SO_4 or Li_2SO_4 , 1 M in distilled H_2O. The cell dimensions depend upon the size of the aluminum sample, but are not critial otherwise. A d.d.p. of 12 volts and a constant current of 100 mA were utilized.

In this manner, hydrogen entering aluminum existed there as protons, essentially at rest. The diffusion, d, of protons in aluminum was considered to be around 10^{-15} m^2/s and the depth, L, was estimated on the basis of the simple diffusion law $L=(\pi\delta t)$.

During the electrolytic process, a beam of electrons was forced to hit the aluminum cathode, and so induced a direct interaction of the protons and the electrons. A radioactive source of $^{90}Sr + {}^{90}Y$ was utilized, having activity about 2.39 KBq (overall uncertainty ± 10%). The radioactive source was a disk having a diameter of 20 mm with implanted $^{90}Sr + {}^{90}Y$ by electrodeposition. It was fixed external to the cell and the electron beam was collimated in order to hit directly the surface of the aluminum cathode with a geometry estimated to be 2π at a good approximation.

Maximum energies E_β of the electrons were considered to be 0.54 MeV and 2.27 MeV respectively for the two isotopes with $\overline{E}_\beta$ of 0.20 MeV and 0.93 MeV respectively. The loss of energy in H_2O for the electrons of 1 MeV was considered to be about 200 eV/μ, and the penetration depth of these electrons in aluminum was calculated to be about 0.4 cm. Bremsstrahlung was also considered in order to estimate the actual energy of the electrons hitting the protons. The electrons were forced to be very close to the cathode. No particular effect was expected for the electrolysis owing to the presence of the electrons.

Concerning the neutron detection, we used a low-level BF_3 detector having cylindrical geometry with a 100 mm basic radius and 250 mm height. The BF_3 detector was connected to a multichannel analyzer, Canberra S20 assisted by a computer, for recording neutron events only, not energy. In fact, as it is well known, the BF_3 counter is sensitive to thermalized neutrons and all information regarding the neutron's original energy is lost since it utilizes the thermal-neutral capture process

$$n + {}^{10}_{5}B \rightarrow {}^{7}_{3}Li + \alpha$$

The BF_3 counters were positioned in direct contact with the external surfaces of the cell and thus with a geometry able to collect all the emitted neutrons. In these conditions, the geometry was considered to be 100%. Sixty neutrons/hour were counted during an active experiment, versus the case in which the experiment was performed without using the radioactive source (two to three neutrons/hour).

Before starting the experimentation, some measurements concerning the neutron background were performed in our laboratories using the instrumentation above. The background controls were executed for several months and a value of two or three neutrons/hour was obtained.

The experiment gave the following results:

a) A mean value of 60 neutrons/hour (overall uncertainty 8%) was measured during the experimentation by the BF_3 detector placed to measure neutron emission from the electrolytic cell; all the measured neutrons were detected by the multichannel analyzer.

b) An anomalous production of heat was observed during the experimentation so that the aluminum cathode melted entirely after one hour of experimentation.

c) This melting did not occur during control experiments with no radioactive source present.

The experiments giving the results (a) and (b) were repeated in our laboratories thirty times, and they always gave reproducible results.

We should add that the aim of the experiment was to measure the possible synthesis of a proton and an electron into a neutron. It was not our purpose to effect calorimetric measurements. It was an unexpected result that we discovered that the aluminum cathode melted in the water during the experiment.

All previous calculations that we performed, accounting for the radioactive source at such low activity, excluded the possibility of the melting of the cathode. The aluminum cathode melted *only when we used the radioactive source and not in the course of the same experiment but without the beta source.*

We were not equipped to follow and measure the temperature rise in the cell. In a repetition of the experiment we are considering measuring parameters to investigate the melting of the cathode.

Conclusions

We are convinced that we observed the cold fusion of protons and electrons into neutrons. In 1920, E. Rutherford[1] introduced the historical hypothesis of the neutron viewed as a compressed hydrogen atom, and we developed a theory[2] in which the proton can be made to interact with the electron via a non-local, non-potential interaction due to the mutual overlap of the wavepackets.

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In the experiment we used an electrolytic cell for a target. The aim was to have a deliberate combination of protons and electrons in a physical situation that promoted wavefunction overlap. It is such overlap of the wave functions between the electron and the target that, we believe, causes the process. Considering the formation of the neutron, we have to exclude here the possibility that the process was due to free electron capture decay of a proton creating a neutron and a neutrino in the final state. The Q-value for this process is -781 KeV, so the electron cannot be captured from a bound state, but must have a threshold kinetic energy greater than 781 KeV. The cross-section for free electron capture decay is extremely small. We do not have knowledge of previous calculations of this cross-section, but we may expect it to be similar to the cross section for the reverse process, neutrino capture by a nuclear target with emission of an electron. In fact, at this threshold value of the energy, the Coulomb potential may be ignored. Neutrino capture cross-sections at about 1 MeV above threshold are less than 10^{-20} barns[3].

We may attempt to state what the measured cross-section for our process was. A 2.4 KBq beta source was used. Considering that the solid angle of the 1 mm^3 target from the source was 2π, the electron flux was $2.4 \times 10^5\ cm^{-2}s^{-1}$. As hydrogen concentration in the target we may assume that of the water, $N_H = 6.7 \times 10^{22}$. Also assuming the detection efficency of recoil neutrons was 100%, then the obtained neutron rate of 60 per hour corresponds to a cross-section of roughly

$$\sigma = \frac{(1.7 \times 10^{-2})\ neutrons/s}{(2.4 \times 10^5\ cm^{-2}\ s^{-1})(6.7 \times 10^{22}\ cm^{-3})(10^{-3}\ cm^3)} = 1.4 mb$$

which is more than seventeen orders of magnitude higher than expected. Thus, the explanation that we have given for such process seems correct. A neutron is formed by the cold fusion of a proton and an electron.

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"A Re-Interpretation of BCS Theory in Superconductivity by using Biquaternion Quantum Mechanics." (Submitted for publication), *Physics Essays* (authors: E. Conte and M. Pieralice)

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pounds, plus the lifting of a 70 pound weight, and the driving of a full size archimedes pump for raising water, could not detach the magnets from the attractive steel bars. The load would have been far too much to sustain the magnetic attraction.

3) Where did they hide the driving wheel? Rooms on either side, above and below were searched.

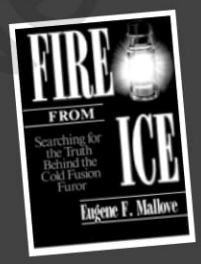
4) How did they effect the translocation of the wheel from one set of bearings to the next? Perhaps they translocated the drive wheel at the same time, hidden within a secret compartment. Did they have two driving wheels? Possibly, but they would still have required to be in close contact all the time. Remember that spectators were allowed to walk all around the wheel to examine it. Where did the drive wheel go at this time?

5) Steve admits that his drive wheel suffered from constantly getting out of synch with the driven wheel; one of the frequent comments about Bessler's wheel was its great eveness of rotation. It never deviated from its regular revolution at any time.

6) Steve describes how he has to time the speed of the drive wheel carefully to the speed of the driven wheel; if the claims he makes are correct how was the invisible drive-wheel turner able to do this if he was in an adjoining room? Thus unable to even see the wheel he was supposed to be driving.

7) He says that his machine is made in an easily knock-down format to enable travel to different towns. Why would this be necessary? We know from descriptions in newspapers, reports and letters that each wheel was different in dimensions and that the inventor was paranoid that someone was going to steal his machine. That was why he destroyed it utterly before each move; to prevent someone getting hold of a vital part and discerning the whole mechanism.

Steve your idea is clever but it doesn't account for any of the above and there are other reasons for believing that Bessler's wheel was genuine. What about Karl the Landgrave of Hesse? He saw the internal workings and stated them to be genuine. He was a man beyond reproach. What of the great Gottfried Leibniz? He examined the machine personally. Do you really think that the man who designed and made a calculater in the 17th century which could multiply, divide, raise to a higher power and extract roots, would not have seen anything as obvious as the magnetic drive wheel that you propose? What about all those people who tried to discredit the inventor over a period of ten years? None of them succeeded. In the end they had to make up a bit of arrant nonsense to explain away Bessler's machine, a nonsense that is palpably false and an impossible explanation. Thank you for your offer to sell me the results of your work, but I think in the end you may have helped to persuade many that Bessler was genuine, so I must decline it.



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Physical Laws of the Emerging New Energies as Predicted by Hadronic Mechanics, II: The New Mechanics*

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This series of articles is further evidence of the current leading role by corporations in the support of truly innovative research which has become impossible in the contemporary academic-governmental community. —R. M. Santilli

1. Introduction

In the preceding paper [I, (*Infinite Energy* No. 22)], I have presented numerous insufficiencies of quantum mechanics establishing beyond scientific doubt the need for a broader theory (this was Objective 1 of Section I-1.3).

In this Paper II, I outline Objectives 2, 3, and 4 of Section I-1.3, namely, the general inconsistency of broader theories and the need for a novel mathematics. I then present the main structural lines of a generalization-covering of quantum mechanics called *hadronic mechanics* [I-3] which was built precisely to resolve the *insufficiencies* of quantum mechanics of Paper I as well as the inconsistencies of other generalizations.

A main objective of this paper is to outline the basic formalism of the new mechanics, with particular reference to its need, its mathematical consistency, its verification of known physical laws (although derived from a broader formalism), uniqueness and invariance properties. Existing applications, experimental verifications, and applications to the emerging new energies shall be presented in subsequent papers of this series.

In inspecting the formalism of hadronic mechanics, the reader should not feel disoriented by the new mathematics, because I provide in Section 9 simple rules for the explicit construction of the theory accessible to anybody with a rudimentary knowledge of quantum mechanics. These results will render the applications of the new theory quite simple, as we shall see in the next papers. The formulation via the new mathematics is needed essentially for consistency and rigor.

Moreover, the reader should not undertake excessive effort to "understand hadronic mechanics," because its value is essentially pragmatic, and thus reduced to its practical effectiveness. To be explicit in this important point, I must confess that, despite decades of studies, I cannot honestly claim that I "understood" the Hilbert spaces of quantum mechanics, because their infinite dimensions are beyond the capacity of the human mind. The point here is that my inability to understand the mathematics of quantum mechanics is irrelevant. The important aspect is that the quantum formalism works well within the conditions of its applicability.

Essentially the same pragmatic approach is suggested when considering the more general formalism of hadronic mechanics. In fact, even though I proposed it, I cannot claim to "understand" the hyper-Hilbert spaces with multi-valued warps, each possessing infinite-dimensions. The important point is that the hadronic formalism works better than the quantum formalism for broader physical conditions, while admitting the latter as a particular case for simpler physical conditions.

A misconception, generally due to insufficient technical knowledge (or bad faith), is that hadronic mechanics is intended to "eliminate" quantum mechanics. The emphasis of Paper I has been that of identifying the limitations of quantum mechanics, and not its "inconsistencies."

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Rather than "eliminating" quantum mechanics, in this Paper II, I assume it instead as the very foundations of the generalized theory. In fact, hadronic mechanics merely provides a "completion" of quantum mechanics much along the historical argument by Einstein, Podolsky, and Rosen [I-5a].

Specifically, I assume that quantum mechanics is valid at long distances under which particles and their wavepackets can be sufficiently well approximated as being point-like. I then assume that the theory needs a "completion" only at short distances of the order of 1 fm = 10^{-13} cm for which the extended character of the wavepackets cannot be ignored. The very meaning of the name "hadronic mechanics" is that of representing hadrons and their strong interactions whose size and range are precisely of the order of 1 fm. We reach in this way the following:

MAIN ASSUMPTION 1.1: Quantum mechanics is assumed to be valid for all distances sufficiently bigger than 1 fm, while for distances of the order of 1 fm or less, hadronic mechanics is assumed to provide a better approximation.

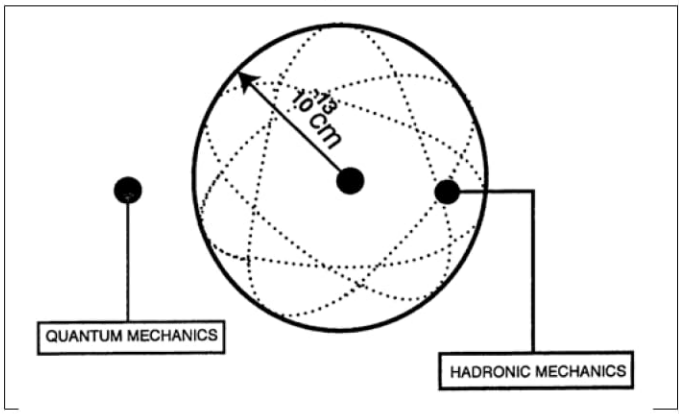

Figure 1. A schematic view of the hadronic horizon, namely, of the sphere of mutual distances of particles with radius 1 fm, outside of which quantum mechanics is accepted as valid and inside of which hadronic mechanics is adopted.

Moreover, I have indicated at the beginning of Section I-2 that, despite the insufficiencies, the axiomatic consistency of quantum mechanic is "majestic." I am here referring to the mathematical, theoretical, and experimental compatibility of the various articulated formulations composing the theory.

The abandonment of such an axiomatic structure, as done by other generalizations (see Section 4) creates the rather serious problems of identifying new axioms, proving their consistency, and establishing them experimentally before any application may acquire a physical value. These tasks are of such a complexity as to go outside the lifetime of any research group. We reach in this way the following:

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MAIN ASSUMPTION 1.2: All abstract axioms of quantum mechanics are assumed as being everywhere valid, including in the interior of the hadronic horizon. Hadronic mechanics is therefore constructed in such a way to merely provide "new realizations" of established quantum axioms.

Another criticism of hadronic mechanics, generally due to insufficient technical knowledge (or bad faith), is that the experimental validity of conventional quantum laws and principles (such as Heisenberg's uncertainty principle, Pauli's exclusion principle, causality, probability laws, etc.) is evidence of the *sole* applicability of quantum mechanics.

This brings into focus the following:

MAIN ASSUMPTION 1.3: When properly formulated and restricted to closed-isolated systems verifying conventional total conservation laws, conventional quantum laws and principles are assumed to be exact everywhere including in the interior of the hadronic horizon.

As we shall see, hadronic mechanics verifies indeed all known physical laws, including total conservation laws, Heisenberg's uncertainties, Pauli's exclusion principle, etc., although they are derived from a broader formalism. In different terms, we shall see in this Paper II that the same physical laws are admitted by *two* different theories.

The verification of known physical laws is also important for credibility, because no deviation from these laws has been experimentally established at this writing in a final form *for closed-isolated systems*, such as particle reactions examined in their center-of-mass.

As we shall see, the case of the physical laws for *open-nonconservative systems* (*i.e.*, systems interacting with another system considered as external), is dramatically different, e.g., because of the lack of validity of conservation laws by assumption, consequential inapplicability of conventional symmetries, and consequential collapse of the entire physical structure of quantum mechanics. After all, the latter theory was conceived for closed-isolated systems strictly verifying the conservation laws of all total quantities and, as such, it cannot be claimed in any credible way to be applicable to open-nonconservative systems.

As an additional introductory comment, I should indicate that by no means do I claim that the generalization of quantum into hadronic mechanics is the only possible generalization, because the beauty of scientific inquiries is their polyhedricity.

Nevertheless, the reader should be aware that, on one side, hadronic mechanics has been proved to be "directly universal," that is, holding for all infinitely possible (topologically well-behaved), nonlinear, nonlocal and nonpotential-nonhamiltonian-nonunitary generalizations of quantum mechanics (universality), directly in the fixed coordinates of the experimenter (direct universality).

On the other side, any other broadening of quantum mechanics should verify the same basic criteria of quantum and hadronic mechanics, such as verify causality, probability laws, invariance, etc. To be novel, other generalized theories should then be proved to be nonequivalent to hadronic mechanics , which appears to be a rather difficult (if not impossible) task.

Recall from Section I-1.2 that *I am solely interested in the study of new energies which are not predicted by quantum mechanics* (Definition I-1.2.1). As we shall see, a main feature of hadronic mechanics is precisely that of predicting "new energies" as per this definition, as well as permitting their quantitative treatment as per footnote I-2 (*i.e.*, mathematical treatments predicting invariant numerical results which can be tested experimentally). These features are the very central motivation of this Paper II.

I should also indicate that, despite the volume of research conducted to date on specific applications, studies on the applications of hadronic mechanics to new energies are still at their beginning and so much remains to be done. Any critical remark on the presentation, content or references of these papers would be gratefully appreciated.

As stressed in Paper I, it is my deep conviction that no really basic advancement of scientific knowledge is today possible in the contemporary academic-governmental complex without a joint consideration of the current problems of scientific ethics and accountability under public support.

Since this misbehavior constitutes clear violation of the U.S. Code of Laws in numerous cases, I believe that the resolution can only take place in the U.S. Courts, no matter how high ranking the responsible academicians are and how powerful their universities are. Lacking such judicial or other clear resolutions, we fail in the very meaning of "The United States of America" and its beautiful Constitution.

In the hope of assisting in the future implementation of these corrective measures, in the main text of this paper I present scientific topics, while in footnotes (or in a subsequent essay) I outline representative cases of my personal experience in the contemporary academic community. No mature appraisal of the former is possible without at least some knowledge of the latter and, in any case, the suppression of the latter would constitute an act of cowardice, which is simply incompatible with my conception of life.

2. Origins of the Insufficiencies of Quantum Mechanics

The insufficiency of quantum mechanics, which is fundamental for new energies, is *the lack of any qualitative or quantitative description of the conditions of deep overlapping of the wavepackets of particles* (Figure 2).

In fact, these are precisely the conditions occurring in the overlapping of the wavepackets of: the two electrons in the helium atom; the bound state of a proton and a neutron in the deuteron; electrons in the Cooper pair; valence electrons in molecular bonds; etc.

Quantum mechanics is strictly: 1) *linear*, that is, depending on powers of the wavefunctions of the first order only 2) *local-differential*, that is, solely valid for a finite number of isolated points and 3) *potential*, that is, solely admitting of forces derivable from a potential V(r). The theory is consequentially: 4) *Hamiltonian*, that is, describable via the sole knowledge of the Hamiltonian operator $H = K + V = p^2/2m + V(r)$); and, therefore, 5) *unitary* when defined on a conventional Hilbert space $\mathcal{H}$ over a conventional field of complex numbers C.

By comparison, the deep overlapping of wavepackets is known to be: 1) nonlinear, because dependent on powers of the wavefunction bigger than one; 2) *nonlocal-integral*, because extended to the volume of overlapping which cannot be exactly reduced to a finite number of isolated points and 3) *nonpotential*, because the interaction is of "contact/zero-range" type, thus prohibiting any description via an action-at-a-distance potential. Consequently, the conditions here considered are: 4) nonhamiltonian, in the sense that they are not entirely describable with a Hamiltonian and 5) *nonunitary* (namely, violating the condition of unitarity when defined on a conventional Hilbert space $\mathcal{H}$ over a conventional field C. The insufficiency of quantum mechanics for a quantitative description of wave-overlapping is then beyond scientific doubt.

When exposed to the physical reality of wave-overlapping, orthodox physicists dismiss its nonlinear, nonlocal and nonpotential character by quoting celebrated experiments of one type

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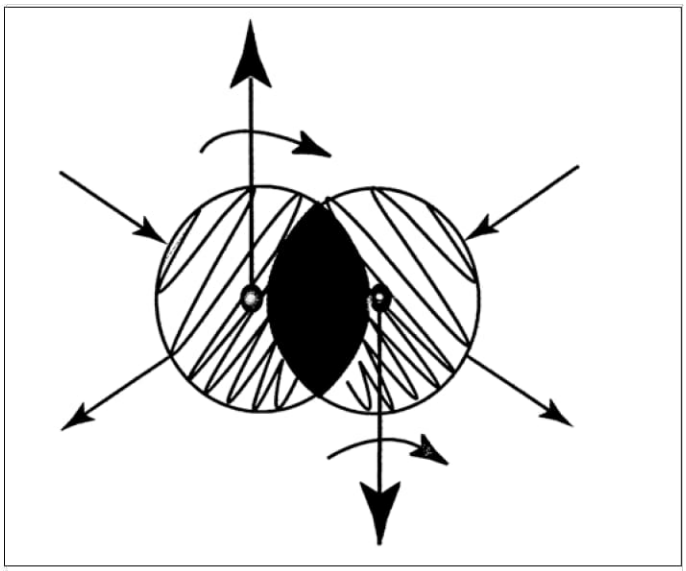

Figure 2. A schematic view of the effect at the foundation of hadronic mechanics: the deep overlapping of the wavepackets of particles at short distances (of the order of 1 fm), which is outside the descriptive capacities of quantum mechanics.

of another, for the evident intent of preserving their beloved theories against possible basic advances. Researchers in new energies should not be intimidated by this show of apparent knowledge. Instead they should subject instead these claims to severe ethical and scientific scrutiny.

The indicated famed experiments are those based on quantum electrodynamics, which do indeed provide experimental evidence of point-like structures, *e.g.*, for the case of the electron. The deception is that this refers solely to the charge structure, which is indeed point-like for the electron. The experiments have no connection whatever with the nonlocality studied in these papers, because the latter is due to the deep overlapping of the wavepackets of the particles, the charge remaining purely point-like.

Similar arguments hold for the remaining features of the conditions of Figure 2.

A deeper inspection establishes that no experimental information is currently available on the physical conditions of wave-overlappings, because all data elaborations have occurred via strictly linear, local, and potential theories (the scattering theory). Therefore, the numerical results of said elaborations can only be compatible with the assumptions.

The issue can be scientifically addressed via the re-elaboration of exactly the same experimental data of quantum electrodynamics permitted by the new scattering theory of hadronic mechanics (isoscattering theory [I-3c]), which has been constructed to admit additional nonlinear, nonlocal and nonpotential effects. A comparison of the results in the elaborations of the same experimental data via two different theories can then resolve the issue in a scientifically credible way beyond the level of personal conjectural feelings by seemingly distinguished academicians.

In summary, the primary insufficiency of quantum mechanics, nuclear physics, superconductivity, chemistry and other contemporary disciplines can be identified in their mathematical structure which is strictly linear, local, and potential, while actual structures of the physical reality have additional nonlinear, nonlocal, and nonpotential effects.

Editor's Note: In a forthcoming issue, we are hoping to present the highly mathematical section of this paper (most likely this will be in issue #25).

Kinetic continued from page 29

of air speed, which resulted in a CFM of 583.1 (sigma = 17.7, 3%). The input was 73.185° F (sigma = 0.9008), output was 92.862 (sigma = 0.1609). Average power in = 3090W. COP = 1.18.

Run 3 for 12/8/98 was a calibration run, complete with another air speed measurement for a CFM of 588.2 (sigma = 19.8 or 3.3%). Jed notated "huge difference between inlet TC readings." The intake temperature is 73.282 (sigma = 0.9007), output was 90.3465 (sigma = 0.1609) for a delta-T of 17.064° F. Average power in = 2990W. COP = 1.0532. So, Runs 2 and 3 together present a solid, if not repeated, result. Power different between the two runs, but the difference in COP's is very interesting.

Run 4 for 12/8/98 was another calibration run with the input thermocouples moved to better positions. Intake temperature = 71.62 (sigma = 0.1283), output was 90.6669 (sigma = 0.1056) for a more credible delta-T of 10.046° F. Average power in = 3,000W. COP = 1.098.

Note added by Eugene Mallove

On January 4, 1999, just as we were going to press, Ralph Pope called our office to report having achieved that day a COP of about 2.0 with the same Kinetic Furnace that Jed and Ed were testing in December. This COP was based on a temperature delta-T of 18° F—60° F inlet, 78° F outlet. This high COP figure was achieved after using newly-obtained water from his well. The high delta-T occurred, he said, only on the third run with this water, thus appearing to show an *onset* of anom-

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Erosion of Freedom in the Scientific World

Janie Axelrad

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We would all expect scientists to have total freedom. Without freedom we might never have discovered penicillin, aspirin or vaccines.

But the truth is that scientific freedom is dead. It has been killed by a combination of media manipulation, money and bureaucracy.

The latest scare story to hit the headlines is the advent of BSE (Bovine Spongiform Encephalopathy) and its links to human CJD (Creutzfeldt-Jakob Disease). You may think that the risks justify the panic, the slaughter of millions of healthy cattle and the near destruction of small farms, but do you really know the facts?

When BSE was discovered, few scientists were studying spongiform diseases, those that turn the brain into a sponge. Because of this, the government consulted those working on scrapie, a similar disease that has affected sheep for over 200 years yet has never caused human health problems. It was easy to hypothesize that scrapie had crossed the species barrier into cows, and could possibly do the same in humans.

At this time, the CJD Surveillance Unit, set up to monitor the disease that affected only one person in a million, was under threat of closure. After announcing the possibility of BSE infecting humans, however, the unit fortunately discovered the new variant of CJD among younger victims. The unit and the jobs of its staff were saved.

Prediction Not Realized

Despite the predicted "epidemic of biblical proportions" proving unfounded, these scientists were elevated to positions as "experts," and were repeatedly interviewed by the media. It was a great scare story, and the "celebrity scientists" wallowed in the attention, while pushing their vision of the truth in every newspaper and TV program.

The same scientists were placed on every committee investigating the problem, including funding bodies. It was easy to deprive dissenters of the funds to prove their case. Anyone finding independent finance to investigate other reasons for BSE is quickly discredited.

The EU (European Union) then stepped in, with a grant of approx £100m, to investigate BSE. All is directed at the "infective feed" theory. This "generous" gift of taxpayers' money allowed the EU to control what was initially a British problem. The Commission banned British beef exports, although we were still allowed to eat our own beef.

Dissenting scientific evidence was ignored as increasing restrictions were placed on our beef industry. None of the regulations seemed to apply to the rest of the EU. The blame for the epidemic was placed on changes in the way our meat was rendered, supposedly allowing the "infective prion" into feed which was fed back to our animals. The same processes were used in other countries without problems. The fact that we exported enormous quantities worldwide where there is no BSE problem is ignored, as is the discovery that earlier rendering methods would not have inactivated the protein.

In the Republic of Ireland the numbers of BSE cases are increasing each year, coinciding with a fall in British cases. So great is the problem that Russia, their main export market, is considering a total ban on imports. The next largest market for Irish beef is the U.K. We are allowed to consume beef that the Russians won't touch. Ireland is a net recipient of EU money, so the lack of action is not surprising.

Even more disturbing is the discovery of BSE in the Irish government's research herd, the last place expected to have fed illegal animal protein to its animals. This herd is partly maintained by the EU.

Significantly, the EU also banned beef from Switzerland, the only other country at the time to have any significant BSE cases. The numbers that caused the ban on Swiss beef have been surpassed in Ireland without comment.

Recently, changes were made to general scientific funding which have completely eliminated innovative study. To receive funding, scientists now must prove collaboration with groups in at least one other EU country. This hardly encourages original thinking, yet most of the greatest discoveries have been by single-minded pursuit of controversial ideas.

Expense Not Justified

The taxpayer supports this unaccountable research. With regard to BSE, apart from the EU research grant and the maintenance of the CJD Surveillance Unit, costing millions each year, over £2bn has subsidized culling healthy cows for no reason except to appease Brussels. If the scientists are wrong, we can't ask for our money back.

The use of money has been seen to suppress dissenting views in other areas. AIDS research commands billions of dollars. Yet dissenters to the theory that the HIV virus alone causes AIDS have been repressed by media blackouts, funding withdrawal and character assassination. Evidence that may have prevented many of the thalidomide cases in the 1960s was similarly suppressed, with disastrous consequences. We have learned nothing since then.

The corruption of science has led to stagnation in research. Only when financial control is removed from those with vested interests will scientists regain their freedom.

The author, Janie Axelrad, is a biology graduate and experienced toxicologist. She is about to embark on formal studies of alternative causes of BSE as the subject for a Ph.D. thesis.

Inside civilized society, politicians are a major problem. In courting popularity for self advancement, politicians almost invariably break the rules of contract by offering what they do not own at a price which does not reflect the real cost of the resources.

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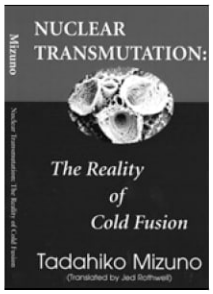

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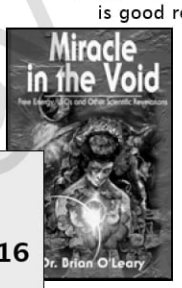

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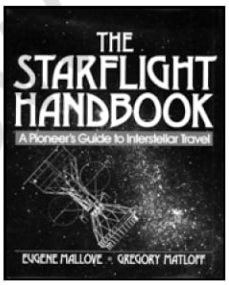

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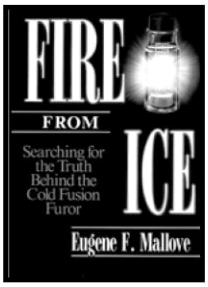

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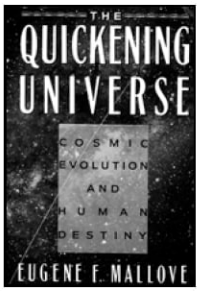

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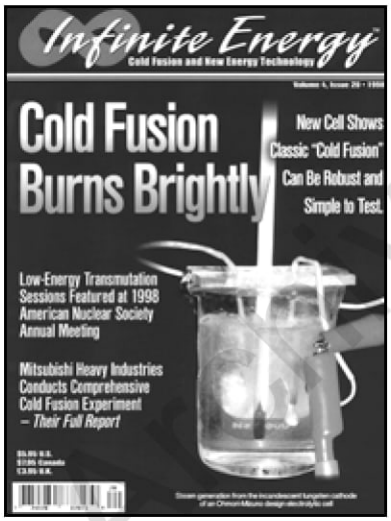

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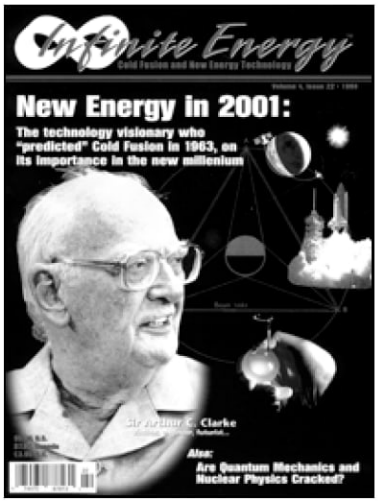

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